THE JOURNAL
OF
Foreign Medical Science
AND
LITERATURE.

VOL. III. APRIL, 1823. No. 10.

Selected Papers.

ON VARICOSE VEINS, WITH CASES, BY E. A. LLOYD, ESQ.

(From the Quarterly Journal of Foreign Medicine and Surgery.)

To the Editor of the Quarterly Journal.

Falcon Square, January 12, 1822.

Sir,—As the safety, and therefore, the propriety, of dividing varicose veins, is still doubted by many respectable surgeons, and as an equal degree of uncertainty appears to exist among those who admit the propriety of it, about the best mode of performing the operation, and also as the nature of the diseases connected with a varicose state of the veins, does not appear to be generally understood, I am induced to send you the following observations and cases, which, I trust, will not be considered by the readers of your valuable Journal, as an unimportant addition to this branch of surgery.

It will be recollected, by many of your readers, that a few years ago, a paper, by Mr. Brodie, was published in one of the volumes of the Medico-Chirurgical Transactions, describing a new method of dividing varicose veins. The operation there described, is that which was employed in the following cases; and, therefore, all the merit that is due, entirely belongs to that truly scientific surgeon, whose name I have mentioned above. It is an operation which possesses in my opinion, many and decided advantages over every other, that has been proposed for a similar object. It is performed with very great facility, occupying very little more time, than the common operation of phlebotomy; the pain which it occasions is commonly very little

severe, and always but of short duration: it affords certain and almost immediate relief, in those painful and tedious ulcerations so often produced by diseased veins; and it is, I believe, when judiciously performed, perfectly free from danger. Before I became acquainted with this mode of dividing varicose veins, I really dreaded to witness the performance of any operation on them, as a fatal termination was so frequently the result; but now, whenever an operation is required, I proceed to perform it without any such apprehension. That I am justified in forming this opinion, I think, must be admitted, when I state, that I have performed the operation in fifteen cases, without any bad symptom being produced, and with the most complete success. In several cases, too, the operation was performed, not only once, but a second, third, and fourth time. I have operated also on the trunk of the saphæna major, as well as on the branches, and equally without any bad symptom supervening. When first I performed this operation, it was exclusively for the relief of those old standing ulcers of the legs, which I had always been taught to consider, as depending on a mere varicose condition of the veins; and as there were often several distinct varices in the neighbourhood of the ulcer, I selected, what appeared to be their trunk, and divided it in the manner directed by Mr. Brodie. In doing this, I hoped to remove, by one operation, the general varicose state of the veins below; but I soon found that I was attempting too much, and that it was essentially necessary to follow Mr. Brodie's plan, and to divide the varices themselves. I, moreover, was induced to believe, that the ulceration, or other disease of the common integuments, is not, as is commonly supposed, the effect simply of the distention of the veins, but that the varix itself first becomes diseased, excessively painful, and tender to the touch, and then, though not till then, excites inflammation, and consequent disease in the contiguous parts.

That the ulcers dependent on varices, are not occasioned by the simple mechanical action of the distended veins, must appear, at least probable, from the circumstance of an excessively varicose state of the veins often existing for years without any ill effect whatever. The success attending the division of diseased varices, also proves the same point; for if the veins acted merely mechanically, it cannot be possible, that the obliteration of so small a part as is effected by the division of a varix, could, as we certainly often witness, remove the whole of the attendant disease: nor could the general varicose state of the veins remain (as is always the case after the operation) as great as ever without pain, or any other disease being produced. Another fact tending to show, that it is not the mere distention, but the inflamed and morbid condition of the varix, which produces the

disease in the common integuments, is, the obliteration of the varix, not being essential to the cure;—a fact which will be proved in the sequel. When, therefore, an ulcer is supposed to depend on disease in any of the veins, it is of importance to examine with accuracy the state of the veins immediately surrounding it, and if at any spot a varix, or one of the veins, be found painful and tender to the touch, the division of the vein, at that point, will generally be found most beneficial. When it happens that there are several varices in the neighbourhood of an ulcer, and that all of them are painful and tender to the touch, then it is necessary to make a separate section of each varix: but when, as I believe is most commonly the case, there are several varices, and yet, only one or two diseased, then it is sufficient, if only those which are diseased be divided. When the disease is of long standing, it often requires a very attentive examination to discover the diseased varices, unless they are in a very tender condition, as the cellular substance surrounding the veins becomes so much thickened, and the skin so much condensed, and deprived of its elasticity by repeated attacks of inflammation, that they no longer project outwards, as when simply varicose, but the leg, at the diseased part, presents a smooth surface, and appears as if the skin was tightly stretched over it. A careful examination will, however, generally enable us to discover the varix; but when the superincumbent cellular substance and skin, become thickened and diseased, the only evidence we have, is an obscure fluctuation being felt in the varix, by a finger placed above it, while with the other hand the blood is impelled towards it, from the vein above or below. This, together with the pain and tenderness, will sufficiently indicate the situation of the varix, on which it will be most advisable to perform the operation. In other cases, in which the veins have lost their prominent appearance, their situation is easily ascertained, by the thickening of the cellular substance on each side of them, which creates a feeling as if they were lying in a cavity with hardened edges. The one to be divided, is determined as in other cases, by the comparative tenderness on pressure, and without any reference to its being above or below the ulcer.

What is the cause of the inflammation which follows some operations on veins, does not appear quite evident. That the simple division of a vein is not the cause of the inflammation, is pretty clearly proved from amputations, and other operations being never followed by inflammation of the veins, except occasionally when a ligature has been applied, to restrain the venous hæmorrhage, or some other contingent circumstance occurs. It might à priori be supposed, that the inflammation, which so often takes place after the dividing or the tying of the saphena,

was occasioned by something peculiar in the state of the vessel, as it is generally more or less varicose, whenever either operation is performed. But facts seem to prove that there must be some other cause, for when Mr. Brodie's operation is performed, it is always on veins much more varicose, and I believe often much diseased, and, yet, instead of any bad symptoms being produced the pain and inflammation which previously existed are speedily relieved. Now the advantage of Mr. Brodie's operation appears principally to consist in the small wound which it occasions, for the skin covering the vein being left entire, of course the divided parts will retract to a much less extent, and the wound being smaller will obviously require a slighter degree of inflammation to heal it, and, therefore, the danger of the inflammation being propagated along the course of the vein will be proportionably diminished.

That the same fatal result, as sometimes takes place, when the trunk of the saphena is divided or tied, may be occasioned by the division of the branches is proved, by a patient dying in St. Bartholomew's Hospital, of the inflammation which followed the division of the varix in the middle of his leg. In this case an incision was made down to the fascia, by which the skin and vein were at once divided, and afterwards the wound was carefully dressed in the usual method, but as alluded to above, in the course of a very few days, the patient died, and evidently from the effect of the wound. The symptoms were so precisely what are always seen attending inflammation of veins, that it would be needless to relate them here. Moreover, examination after death proved them to be produced by that cause. About the same time there was another patient operated on in the hospital, in the same manner, and though he ultimately recovered, most certainly suffered more severely, and a much longer time than he would, if the skin had been left entire.

Why the ligature and other modes of operating on the varicose veins, should be followed by more severe symptoms, must be so obvious to every one, that I shall not enter into the inquiry. Whether there is greater danger in operating on the trunk than on the branches of veins, it is not for me to decide, but I may add, that having twice operated on the saphena major, without the slightest bad symptom being occasioned, I should not feel the least apprehension in repeating the operation.

As some of your readers may not be acquainted with Mr. Brodie's mode of operating, I will briefly describe it. Mr. Brodie employed in the operation a narrow sharp-pointed bistoury, slightly curved, with its cutting edge on the convex side; but it appears to me that a straight bistoury, with its edge only so convex, as is necessary to bring it to a point, is a better instrument for the purpose, because if it is curved, its point is

liable to run through the skin at the time it is turned, preparatory to dividing the vein. The operation consists simply in dividing the vein and at the same time leaving the superincumbent skin entire; and this is effected by introducing the knife, described above, between the vein and the skin, with one of its flat surfaces turned downwards, till it is carried completely across the varix, when the cutting edge is to be turned downwards, and the vein divided, the knife being withdrawn by the same effort.

After the varix is divided, the treatment I have always employed is very simple. It consists in abstracting at the time of the operation, as much blood as the patient can conveniently lose; the applying a compress to the divided varix, and a common bandage to the whole leg; the keeping the bandage wet so as to prevent any increase of heat in the diseased part, strict confinement to the horizontal position, and in particular attention to the diet, and to the regulation of the bowels of the patient. That strict attention to all these points is always absolutely necessary, I do not mean to assert, as my own practice has proved to me that it is not, but as the safer plan I should always enjoin it, unless there were particular circumstances to prevent it. How long it is to be observed must vary in different cases, but generally I have found a few days sufficient to keep the patient wholly confined to the horizontal position; but the roller should be continued for a much longer time. For fear of exceeding my limits, I shall not enter more fully into the treatment, but proceed to relate in as concise a manner as possible, the particulars of some of the cases in which I have operated.

The first case is that of a man, about 45 years of age, who kept a vegetable stall, at Fleet-market, and who therefore was constantly obliged to be on his legs. Indeed it was a particularly unfavourable case, for the man declared that it was impossible for him to lie up for more than a day, and, moreover, his general health of body was very much impaired. The veins of both his legs were in an excessively varicose state, and connected with those on the right leg were three or four large ulcers, which had existed for thirteen years. In this case, in which my friend, Mr. Stone, of Argyle-street, kindly assisted me at the operation, I first divided the vein a little below the knee, when there appeared to be only a single trunk, expecting or rather hoping that by this means the vein would be obliterated, the weight of the column of blood diminished, and consequently the excessive distention of the varices below removed, as is stated to be the case, when the ligature is applied. About twelve ounces of blood were taken away at the operation, and a compress and bandage afterwards applied, and the treatment

altogether as I have described. The wound caused by the operation healed at once, and did not produce the smallest bad effect. The immediate effects of the operation were to relieve the man of much pain, which he was constantly suffering before, and to produce a decided disposition in the ulcers to heal, although they had resisted every attempt that had been previously made, and especially tight bandaging and strapping, which I had myself applied for several months. These beneficial effects were, however, but temporary, for in less than a fortnight the pain in the leg returned, and the ulcers became again irritable and unhealthy, and the disposition to heal entirely ceased. At the point where the division took place a hard lump had formed, which evidently obstructed the passage of the blood through the vein, but above and below this the blood appeared to be perfectly fluid. The first operation not proving successful, a second was had recourse to. This consisted in the division of a large varix, which was tender to the touch, and communicated with an ulcer that was situated high up in the inner part of the leg. In this instance the treatment was the same as in the former, except that the man walked home directly after the operation, which was performed in the House Surgeon's apartments, at St. Bartholomew's Hospital. From this operation no ill effects followed, and the small wound occasioned by it healed in a few days. The old ulcer, too, connected with the varix which was divided, rapidly healed: and the varix itself lost all its tenderness, but the other varices remained tender as before. Two other ulcers also that were lower down, and were in connexion with the other diseased varices, were not at all benefited, which sufficiently proves that it was not the mere abstraction of blood, which was the occasion of the healing of the other ulcer. For the cure of these two ulcers two other operations were necessary, and subsequently the patient became perfectly well. That it is not the bleeding that cures the ulcer, is further proved, by the fact of no such effect following those immense spontaneous hæmorrhages, which so frequently take place from old ulcers of the legs depending on varicose veins. This case was moreover important in other respects. It shows that though the trunk of the saphena be obstructed, as when a ligature is applied, it does not necessarily follow that the varicose condition of the veins below be removed. It also first induced me to believe that the ulceration is not generally the effect simply of the distension of the veins, but, as I have stated before, of disease previously existing in the varices themselves, and thence propagated to the contiguous parts.

The next case proves almost the same points as the former. It is the case of Daniel Stodart, who was a patient in Darker's

ward, St. Bartholomew's Hospital, whose saphena major I divided just as it passes over the knee, and it particularly shows that the veins below shall remain pervious, though the trunk at one point be completely obstructed. In this case, the veins of the leg were excessively varicose and distended, accompanied by an ulcer, and an extremely painful state of the whole leg. The ulcer was not the effect of a diseased varix, but of an accident which he met with nearly a twelvemonth before. It was, however, prevented from healing by the irritation which was kept up by the excessive distension of some of the varicose veins, and to relieve this the operation was performed. And so far it succeeded; for afterwards the pain at once subsided, the ulcer speedily healed, and the veins became much diminished in size, though they still remained somewhat varicose. No bad symptom whatever followed the operation, and at the point where the vein was divided, a hard lump, of the size of a large walnut, formed, and, as in the former case, the blood above and below it was completely fluid. Between one and two years after this patient left the hospital, I attended him with a gonorrhœa, and to that time the leg had continued perfectly well, and free from pain, and the veins diminished in size, though, as when he went out of the hospital, they were still varicose.

The following case proves, that though the varix be not obliterated by this division, yet its morbid condition will be removed, and at the same time the dependent disease of other parts.

Sarah Special, ætat 29, applied to me in June, 1818, on account of a large ulcer on the inner ankle, which had existed for above six years. It was extremely painful, and the skin around it was much inflamed and excoriated. The leg too was much swollen, and she informed me it constantly remained so. She had been in one of the Borough hospitals, and in the London Hospital for many months, but had never been able to get the ulcer healed. The pain occasioned by it was so severe, that she was altogether prevented from following any regular employment, and also her general health suffered very materially. Strapping and bandaging had been employed for months successively, but never produced more than limited relief, and that was only temporary, for as soon as they were left off the ulcer became as bad as ever. On examination, I found, just above the ulcer, a large and somewhat elevated varix, with a hardened margin, which was extremely tender. The skin above it was inflamed, and interspersed with ulcerated clefts. The veins of the leg were generally varicose, but at no other spot were they tender to the touch. This being the case I proposed to the patient the division of the diseased varix, to which she readily consented. I therefore divided it, and in doing this it was necessary from the large size of the varix, to introduce the

knife for above two inches between the skin and vein. About 16 ounces of blood were abstracted; the aperture was closed in the common manner, and healed by the first intention. The ulcer was subsequently treated in the most simple manner, and in less than five weeks all pain and appearance of disease were removed. I have seen this patient repeatedly since that time, and her leg has continued wholly free from pain and disease, notwithstanding she has been constantly actively employed. When I saw this patient last her leg was as well as ever it was, and the vein had no sign of ever having been diseased, though, as I have stated above, it was not completely obliterated.

The preceding cases are interesting not only so far as they demonstrate the safety of dividing varicose veins, but also by showing, that ulcers of the legs, connected with a general varicose state of the veins, will speedily heal, notwithstanding they have previously resisted every other mode of treatment, if merely the varices which are immediately adjoining them, and tender to the touch, be divided. The first case also proves, that although ulcers dependent on varices have existed for many years, they may be as certainly and as speedily healed, as those which have existed only for a much shorter time. The same points are further confirmed by the following case; and it moreover shows, that an ulcer of nearly twenty years standing may be healed in an old person, without any injurious effects ensuing, if at the same time a proper regimen be observed.

Early in the spring of 1818, I was consulted by an old lady, between sixty and seventy years of age, on account of a painful ulcer on the small of the right leg, which occasioned her to be so lame that she could hardly walk about the house. The ulcer, she informed me, had existed for nearly twenty years; it was about an inch and a half in length, and an inch and a quarter in breadth; its edges were hard and elevated; its surface was of a variegated and glossy appearance, and it discharged copiously an offensive oily kind of matter. The skin surrounding the lower part of the leg was of a dark reddish purple colour, seemed tightly stretched over the other integuments, and had that peculiar shiny appearance which so commonly attends old standing ulcers of the leg. The veins of the leg were not much elevated except at the upper part of the calf, but still they were much dilated; and upon examining the parts about the wound with my hand, I distinctly felt a large varix, with hardened edges, and tender to the touch, situated between the tibia and fibula. This varix therefore was selected to be divided, for although there were in the neighbourhood of the ulcer other varices to be felt, still there were none of them either inflamed or tender. In a few days before the operation was performed, the patient was confined to vegetable diet, and all

stimulating drinks were abstained from; her bowels were freely opened, and the limb was kept constantly in an horizontal position. The operation was performed in the same manner as in the preceding cases—about twelve ounces of blood were abstracted at the time, and no bad symptom ensued. The wound occasioned by the operation healed by the first intention; and on the third day, when the dressings were removed, the edges of the old wound were found white, and fibres of skin were seen shooting from the whole circumference. The process of cicatrization, moreover, went on so rapidly, that the old ulcer, which had previously for so many years resisted every attempt to cure it, was, by the nineteenth day, completely healed. The pain, too, which had before been a source of constant trouble, wholly ceased; and indeed the success of the operation was altogether so complete, that the leg remained perfectly well to the time of her death, which happened about three years and a half afterwards. The bandage was worn for several months after the ulcer had healed, but subsequently it was left off. As the ulcer in this case had existed for so many years, I considered it right to direct the patient, always to take some opening medicine two or three times a week, and to be very moderate in her diet. By persevering in this plan of treatment her health not only continued unimpaired, but it was really better than when the ulcer was open.

The rapidity with which old ulcers, connected with a varicose state of the veins, heal after the source of irritation is removed by the division of a diseased varix, is often quite astonishing; and I never saw it more strikingly evinced than in the following case:—A poor man, sometimes employed as a hackney coachman, sometimes as an ostler, had had a large ulcer on his right leg, with a varicose state of the veins, for fourteen years, and it had resisted every attempt to cure it. He had been repeatedly in the different hospitals, and I had known him in St. Bartholomew's at three different times; and at one period his leg was regularly strapped for several months, by a pupil of the late Mr. Baynton, but all without effect. When he was last in the hospital the ulcer was of an immense size, extending nearly all round the leg, and for between three and four inches in the longitudinal direction, and from it there was a most profuse discharge of offensive matter, and frequently of blood. At this period I was house surgeon of the hospital, and attended him under the direction of Mr. Abernethy, and for several months made use of strapping and various other remedies in succession, but without any benefit to the wound. The man had therefore given up all expectation of having it cured; and as it was excessively painful, and his health was suffering very much, he earnestly solicited that his leg might be amputated. Such was the state of the case when

it was proposed, and I was directed, by Mr. Abernethy, to divide some diseased varices which there were adjoining the wound. To this the patient readily consented, but, I believe, without any sanguine expectation of much benefit being derived. The case was, indeed, as unfavourable a one as could well be selected, for the whole of the cutaneous veins of the leg were more or less dilated; the patient's health was much impaired by hard drinking; he was subject to frequent attacks of inflammation, and swelling of the whole leg, which were often attended by rigors and violent sympathetic fever. The varices adjoining the wound were very little elevated or visible, from effusion into the subcutaneous cellular substance, but their hardened margins and excessive tenderness, rendered them easy to be discovered by the touch. There were three of them in a diseased state, all of which I proposed to divide, but at separate periods. The first which I divided was nearly an inch and a half across, and situated partly under the edge of the ulcer. About ten ounces of blood were abstracted from the wound, which had healed by the end of a week, and no bad symptom whatever followed the operation. One of the other varices was then divided, on the same side of the leg, but higher up. The wound occasioned by this operation healed as readily as the first, and about the same quantity of blood was taken away from it. The beneficial effects of these operations were most evident, for immediately after the second, all the pain which had so long and so much tormented the patient, altogether ceased, and the ulcer which had so long resisted every attempt to cure it, began rapidly to heal. Indeed the healing process went on so exceedingly fast, that it was not thought necessary to divide the other varix which has been stated to have existed, and in less than seven weeks the whole of the immense ulcer was entirely healed, and the patient discharged from the hospital. At the time the patient left the hospital, the varices that had been divided appeared perfectly obliterated, and pressure made on those parts occasioned no more pain than on a sound part; but the varix which was not divided was still tender to the touch. Such a state of the undivided varix, at the time of his leaving the hospital, rendered it probable that it might be necessary ultimately to operate on it also; and this was very near being the case, for a few weeks afterwards he returned complaining of swelling and pain in the leg, the whole of which was also much inflamed, but fortunately these symptoms speedily subsided by the use of common measures; and, some months after, I met the man engaged as a hackney coachman, at which time his leg was sound and free from pain.

There is a species of erysipelatous inflammation of the legs to which adults, but especially those about the middle age, who

have a varicose state of their veins, are subject, which I believe is permanently curable by no other means than by the division of the diseased varix or varices. This affection is extremely painful, and as it subsides, leaves the skin permanently discoloured. Considerable thickening often takes place in the cellular substance between the veins and skin, so that the prominence of the varicose veins is obscured, and the diseased varix, as I have stated before, can only be discovered by the touch. In cases of this description, it often happens that considerable swelling attends the inflammation, and that the attacks are frequently repeated, and that the inflammation never entirely, but only partially subsides. In other cases, instead of deposition into the cellular substance, an eruption of small, irregular, hardly elevated vesicles succeeds to the inflammation, or the skin cracks, as it were, or becomes excoriated, from which there is a copious effusion of an acrid serous fluid. In these cases the attacks are not so sudden nor so violent as in the former; but when once this affection has come on it seldom subsides to any great degree, but almost constantly remains a source of great uneasiness to the patient, till, by some means or other, the diseased varix is cured. The ulceration which attends these cases seldom extends beyond the skin, and usually heals as the violence of the inflammation subsides, leaving the skin tender and covered with a kind of scurf. These cases are often so extremely painful, that they prevent the patients using any exertion, and sometimes wholly confine them to the bed, and also the health of patients becomes much impaired from the want of rest and the constant pain which they suffer. The following cases will more fully illustrate this species of disease.

Mary Billin, about 35 years old, was admitted into St. Bartholomew's hospital, on account of a disease of the parts surrounding the ankle and small of the right leg. The disease was so painful that it had obliged her to give up her business, and go into the workhouse. It had been in this state for more than three years. When she came into the hospital the parts about the ankle were much swelled, and of a purple red colour. The skin also was cracked, and superficially ulcerated, from which there was constantly effused a watery fluid, of an acrid irritating nature. Her health also had suffered so much that the whole of the vital functions were more or less disordered. This patient remained in the hospital for several months, during which time every variety of soothing applications were tried; and besides these she was bled from the arm more than once, the greatest attention was paid to her diet, and to the state of her bowels, and she was kept constantly in bed; but all without effect, for when discharged from the hospital she was in no essential respect better than when she was taken in. While she was in the

hospital the disease was considered to be one of those intractable cutaneous affections of an anomalous nature which are occasionally met with; and its connexion with diseased varices was not at all suspected. When she left the hospital she was made an out-patient, and went to reside at her mother's, and it was at this time I ascertained the true nature of the disease. The mere appearance of the leg afforded no clue to the cause of the disease, as the dilatation of the veins was hardly at all visible, but it was readily ascertained by the touch, for on tracing with the fingers the surface of the leg, the veins were found generally dilated and tender, and a large varix, with thickened edges, and exquisitely tender, was felt about the centre of the diseased part. It being stated to the patient that this was the cause of the disease, and that it could only be cured by an operation, she readily consented to have it performed, and accordingly I divided the varix as in the preceding cases. To make a complete section of the varix it was necessary to introduce the knife almost its whole length, which is full two inches and a half. About twelve ounces of blood were abstracted from the vein at the operation, and the subsequent treatment was as usual. The pain occasioned by the operation was severe, but it existed only for a very short time, and after this the patient was much easier than before the operation, and slept soundly all night, which she declared she had not done for above two years before. The success of the operation was most complete, for in less than a week the disease had subsided so much, that very little inconvenience was experienced from it; and at the expiration of a fortnight the patient was able to walk almost as well as if she had never had any disease in her leg. After the operation the health of this patient very much improved, and she followed her regular business, taking a great deal of exercise, for about a year and a half, but at this period she was again laid up by a similar affection of the other leg, and great attendant disorder of her health. To remove this affection, bleeding, bandaging, and cooling washes were employed, but without affording any decided relief, but it was speedily relieved by adopting the same means which were so successful in the former instance. After this, I saw this patient repeatedly, at different periods, and when I saw her last she informed me, that the legs continued well, and free from pain, except after using more exercise than common, when some of the enlarged veins occasionally became somewhat painful.

In four other cases, very similar to the last, the division of the diseased varices proved equally successful. Two of these occurred in patients who had been in the hospital for several months without receiving any benefit. One of them was in Faith's the other in Hope's Ward; and both the patients were

young women, the oldest being not much more than thirty In one of them the disease was precisely as in the last case; but in the other there was a large ulcer, as well as the cutaneous affection. In the former case the patient had been a nurse in the hospital, but on account of the disease in her leg had been obliged to give up her situation, which, however, after the operation, she resumed, and without subsequently experiencing any inconvenience. In the latter case the patient had had, a short time before the varix was divided, a severe attack of inflammation of the whole leg, accompanied by such a high degree of sympathetic fever, that her life was almost despaired of, and yet no bad symptoms whatever followed the operation. The state of the ulcer, too, immediately improved, and the healing, which before had resisted every means which had been employed, took place without difficulty.

The next case occurred in a man of the name of Spiers, between thirty and forty years old, and a gardener by employment. The disease was principally confined to the ankle of the right leg. It had existed for between three and four years, and during the last six months, a small ulcer had also broken out, and altogether the effects of the disease were such that the patient, although a strong and otherwise healthy man, was almost wholly incapacitated from following his employment. His health also suffered considerably. When first I saw him, the whole leg was much swollen, but, as I have mentioned before, the actual disease was principally confined to the parts in the neighbourhood of the outer ankle, and yet the pain he suffered was so severe as to greatly deprive him both of his rest and appetite. The diseased vein was situated between the lower part of the fibula and the tendo Achillis, and was very much dilated, and extremely tender. The operation was performed in this case as in the others, except that the section was made obliquely across, to include as much as possible of the diseased vein, which was of very great extent. My friend, Mr. John Ware, had seen this man before, and therefore kindly assisted me in the operation. As in every other case, no bad effect whatever followed the operation, and the disease for which it was performed, so speedily subsided, that in less than a month the patient was enabled to resume his ordinary occupation. This case could not, however, be said to be complete, because a portion of the vein which had not been included in the section, continued tender to the touch, so that though the disease of the other parts was now cured, its return was rendered probable: and this I stated to the patient, and proposed to him to allow the operation to be repeated; but as he had got rid of his immediate trouble, and the time of the year made it important that he should be in his garden, he would not consent to

it. Since that time, though more than three years have elapsed, a second operation has not been performed, and for more than two years the ankle remained sound; but subsequently the disease has in some degree returned, and the patient himself considers that a second division of the vein will be necessary; and when I saw him last, was quite ready to submit to it, so convinced was he of the benefit he derived from the first operation.

The next case occurred about six months ago. The patient was a middle-aged female, of the name of Whitford, daughter-in-law of the surgeon's instrument maker of that name. She had been subject to a painful affection of her right leg for several years, which during the last year had so much increased, that it had obliged her to confine herself almost entirely to the house. The leg frequently swelled to a very large size, and was generally covered, more or less, by a vesicular eruption. How much the affection, in this case, resembled idiopathic erysipelas, appears from its having been considered as such, and treated accordingly by a physician, who had attended the patient two or three months before I saw her. That, however, it was wholly dependent on the morbid condition of the veins, is proved by the whole of the inflammatory symptoms subsiding upon the division of a very large and diseased varix, which there was on the inside of the upper part of the small of the leg. About a pound of blood was taken away at the operation; and perhaps to this source some of the benefit which followed may be attributed; but it appears to me, that the loss of blood was only beneficial in a secondary degree, because, in this case, the patient had, two or three times before, been bled from the arm without the pain or inflammation being at all relieved; and also, because the good effect of the operation, as far as relates to the veins, was principally, if not wholly, confined to the varix which was divided; the other varices that existed at the same time, though of smaller size, and less diseased, not being materially benefited by the operation, but being still in such a tender state as to render it probable that they will ultimately require division. The loss of blood in this case induced a complete state of syncope, which has been stated to be a rare occurrence, as the consequence of abstraction of blood from a varicose vein.

Although I have stated that the mere abstraction of blood does not occasion any permanent benefit when there is actual disease of a varix, still I am well aware that even then it often affords considerable temporary relief, and that it is really of the greatest service when, from excessive distention, the veins are in a painful condition. I could, indeed, adduce many cases proving the benefit which is sometimes derived from taking blood away from varicose veins.

Before I conclude this paper, I must beg to observe, that it has not been my object to consider the general treatment of varicose veins, but only those points to which I have specifically adverted in the beginning.

4, Falcon Square, January 21st, 1822.

On Hooping-Cough. By JOHN WEBSTER, M. D. Licentiate of the Royal College of Physicians of London, Physician to the Royal Infirmary for Sick Children, &c. &c. &c.

(From the London Medical and Physical Journal.)

Of the diseases common to children, none are so frequent, and few so troublesome, as hooping-cough: indeed, such is the nature of this complaint, that parents look forward to its affecting their offspring with much anxiety, and they are not easy on account of their children till it is over; so much so, that it may be said to form an era in the life of the individual, from which many other diseases and affections, that afterwards occur, are dated,—nay, are sometimes even said to arise.

On a complaint so common, and which every mother almost thinks herself fully qualified to treat, it may perhaps be asked if I have any thing new to communicate, to make it the subject of a paper? my only object, then, in this communication is to recommend the trial of a method of cure, which has been lately very extensively employed, under my direction, in a great number of cases of pertussis I have had an opportunity of treating at the institution to which I have the honour to belong; and, as the marked success which has attended the practice usually pursued induces me to think it really beneficial, I am the more anxious to lay it before my professional brethren, through the medium of this Journal. It will be very gratifying to learn the result of the experience and observations of those who may be led to give it a trial; and, should their experience coincide with my own, one step will be made towards the alleviation of a very frequent source of anxiety to parents and of affliction to the most interesting part of the community. Having this in view will, I am sure, be deemed a sufficient reason for submitting to the public the few imperfect hints contained in this paper; for I am more anxious to communicate what appears to me one useful practical fact, than to compose a regular treatise on this so well known, and already overwritten, subject.

Like the history of many other diseases, that of hooping-cough is enveloped in great obscurity, and it is not yet clearly ascer-

tained whether the ancients were acquainted with it or not. The researches which Sprengel has made on this subject tend to show that the disease was first observed by medical writers in France, in the year 1414; since then it has, at various periods, been occasionally epidemic in every part of Europe. In this country it now almost constantly prevails, but is most common in spring and autumn.

When we meet with a child in a violent fit of hooping-cough, the first thing which attracts our attention is the peculiar sound produced during inspiration, the violence of the cough, and the difficulty of breathing: so peculiar, indeed, is this sound, that, once heard, it can never be mistaken; and no other symptom is necessary to be present to constitute the disease. Although this peculiar sound forms the principal (in the vulgar eye the only,) symptom of the disease, pertussis does sometimes occur without the hoop being at all observed. In these cases, the cough is quite different from any other; and, if I may so express it, the sound appears as if it came from some wooden instrument, having a kind of shrillness only to be known by actual observation. Looking at the symptoms now mentioned, a person will naturally conclude that the disease exists principally, if not wholly, in the organs of respiration; and, of course, that all our efforts are to be directed towards the removal of the affection which is thought to exist in these parts. On a nearer examination of the infant labouring under this complaint, we, however, find that the pectoral symptoms are not the only ones that are present, but are preceded or accompanied by pain of the head, and fulness and redness about the temples and eyes; and that these symptoms occasionally give more inconvenience to the child than the cough and difficulty of breathing, which, to a by-stander, appear to constitute the chief part of the disease. To be convinced that this affection of the head is often very severe, we need only observe the blood-shot and inflamed eyes after a fit, and the severe pain which is almost constantly felt in that organ during and after it; the great and decided relief which epistaxis always produces in this disease; and, lastly, the fact that hydrocephalus does sometimes occur as a sequela to this complaint.

That this affection of the head does more frequently occur than is generally imagined, a number of cases I have lately met with fully convince me to be the fact; and I have no doubt, if children could express their previous feelings and symptoms more accurately than they generally do, we should find this to be so. A case which lately came under the observation of my very ingenious and diligent friend, Mr. Webster, of Dulwich, and which he communicated to me, throws much light on the point now in question: it was that of a young woman, a servant in a family

where hooping-cough was then prevalent, who also became affected with the disease, not having had it before; but, previous to the cough and hooping manifesting themselves, and before the disease was suspected to be coming on, she complained, for a few days, of great pain and fulness of the head, accompanied with stupor and giddiness. So troublesome and severe were these symptoms, that she was obliged to remain in bed for two days, being unable to move about; and, to use her own expression, "she staggered about the house like one drunk." Many corroborating cases of my own might be adduced, but it appears to me to be superfluous to do so.

As hooping-cough is not now so fatal a disease as formerly, not many opportunities offer themselves to enable medical men to examine into and find out the nature or actual seat of the complaint; consequently, on this point, much obscurity prevails, and many opposite and conflicting opinions are entertained. I believe, however, that, although most men think the primary seat of the disease to be in the organs of respiration, scarcely any morbid change has been found in them; at least, sufficient to prove that the complaint actually began in these organs: and, where disease has been found, might it not have been secondary, and not primary? A great deal has been said about spasm of the air-cells, &c.; but, as I do not exactly comprehend these ideas, and as it serves little purpose to enter further into these and the like speculations, I shall leave them to those who may think they merit attention, or who may better understand what this spasm of the air-cells means.

Considerable difference of opinion exists as to the contagious or infectious nature of hooping-cough: some physicians think that it is so; others, that it is only an epidemic disease. On this point, much discussion has taken place, particularly in Germany, and many names of high reputation are ranked on each side of the question; but, it not being my intention now to give a complete treatise on pertussis, I do not think it necessary further to investigate this fact. At present, I will only remark, that, as far as my own experience goes, I am very much disposed to view this disease in the same light as that of common fever,—namely, that its propagation depends on a particular state of the atmosphere, and of the constitution of the individual affected; and not upon contagion, according to the vulgar meaning of the expression. The circumstance of hooping-cough being very much benefited by a change of air, which, with some, particularly in Scotland, is thought to be the best and only cure, tends much to strengthen this opinion; as also the well-known fact that many children become affected, without ever having had any communication whatever with any

children under the disease. At some future period I may, perhaps, attempt to elucidate this point.

Having lately paid a good deal of attention to hooping-cough, and having observed the great influence this affection of the head has upon the disease, I have been thereby led to entertain the opinion that the actual seat of the complaint may be in the head, and that the affection of the respiratory organs is only to be considered as a secondary effect, or as an effort of nature to relieve herself by expanding the lungs to an unusual degree, thereby allowing a greater quantity of blood to flow into them, which may in some degree diminish the fulness and congestion now spoken of. This is a very mechanical theory, but it appears to me not altogether inconsistent, and is somewhat plausible. To the opinion that the seat of hooping-cough is in the head, I have been led from being informed, by my very intelligent and experienced friend, Mr. Wardrop, that he had found, on examining the head of an infant who died of pertussis, very evident marks of congestion and inflammation of the brain and its membranes; and that he thought blood-letting and leeches would have been very useful in this case. Some fatal examples of the same kind, which I have had an opportunity myself of examining, in which there were both signs of previous inflammation and congestion, and one with more water than usual in the ventricles,—also the circumstance that, generally, no marks of disease are found in the lungs,—strengthen this opinion. Besides, every one must often have observed the great relief epistaxis gives in this complaint; and every body also knows, where the child bleeds profusely at the nose, the disease is generally more mild than otherwise; for this is nothing more than, as diarrhœa is in the beginning of fever, an effort of nature to cure the disease. Further, if any reasoning can be allowed from the success of particular treatment, certainly the result of the plan I have lately pursued, and which is nothing more than simply doing by art that which nature occasionally does of herself, confirms me in the opinion I have lately taken up, and have endeavoured to point out and to elucidate. However, as I may be mistaken, and as I should wish to have more experience and morbid examinations, either of my own or of others, before boldly asserting as a fact what I now suspect to be the case, I shall pass on to detail the treatment I employ, which, whatever may be the pathology of hooping-cough, certainly has, in my hands, been most successful: it is simply to follow the dictates of nature, *by relieving this congestion in the head* that has been already shown to exist; whether primary or secondary, it remains for future experience and investigation fully to establish.

The best way to carry this object into effect, is to apply, as

will be naturally concluded, leeches to the forehead and behind the ears; for there the blood being drawn from parts supplied by the ramifications of the supraorbital arteries, and of those of the internal ear, the blood is drawn more directly from the head than it would be were the leeches applied to the temples. This observation will be found fully exemplified in cases of common fever with a determination of blood to the head, where leeches applied to the forehead do often a great deal more good than when applied to any other part.

Upon the same principle, the jugular vein may be opened with much advantage. I have not myself had recourse to it in any case, because I have always got enough blood in the way already mentioned: besides, as the children of the poor are very likely to be neglected, I would not wish to trust to the parents, lest they should, from carelessness, allow the bandages to slip off, and much blood might thereby be unnecessarily lost. However, I have no doubt but that it would be very useful; and, in private or hospital practice, I would have recourse to it in obstinate cases.

General blood-letting is also very beneficial; and, if we look into some of the older, as also the modern, writers, we shall find it very much recommended, particularly if the patient be of a full inflammatory habit of body, although the theory which led to its employment be different from the one here advanced. It is a remedy which may be had recourse to with advantage, especially where the patient is plethoric or affected with any general febrile action; the leeches being also employed for the removal of the head-ach and other symptoms.

In addition to the evacuation of blood by applying leeches to the forehead, I keep the bowels open, by moderate and frequent doses of calomel and rhubarb, or calomel and ipecacuanha, and give squill and antimonial mixtures for the cough; but these remedies are of secondary importance, and only used as occasion requires: the leeches are what I principally, and often entirely, depend on.

As cases sometimes occur where, from the long continuance of the disease, and the consequent exhausted state of the little patient, any loss of blood would not be advisable or prudent, I then substitute blisters behind the ears and to the nape of the neck, and give digitalis: this medicine, besides its peculiar action on the heart and arteries, has also a *local effect* (if I may so express it,) in diminishing the determination of blood to the head; and I am inclined to think so, as well from the opinion of the celebrated Professor Borda, of Pavia, as from having observed its great efficacy in the cure of diseases in adults accompanied with a disposition to fulness of blood in the head: it

may, therefore, be used in those cases of hooping-cough where any loss of blood is not thought prudent.

The success I have met with in the treatment of pertussis may, perhaps, not be altogether credited; but I would only request practitioners to make trial of the method of cure now recommended, to see its efficacy. I have been often very much gratified, nay, even surprised, at the great, and often immediate, relief which the application alone of a few leeches to the forehead has produced. I have frequently met with children affected with this malady for four or five weeks, come back a few days after the leeches were employed, and their parents tell me that the hooping ceased soon after the blood had been evacuated, and that they now only had a little cough; which also generally yielded, in a few days more, to the use of purgatives and saline and antimonial medicines, or to a second application of the leeches.

Here it may not be uninteresting to observe, that many medical men are of opinion that pertussis, like many other contagious diseases, must run through a certain course; and, consequently, that little can be done either to check its progress or to cut it short altogether; and all that we can hope to accomplish is to be able to moderate its violence. The opinion that little can be done in this disease, I am very much disposed to hold not only as highly detrimental to the patient, and a complete bar to farther improvement in medical science, but as highly erroneous, although supported by the great authority of Sydenham, Werlhof, Hufeland, &c. My own experience has fully convinced me that this disease may be cured at any period of its course; and that it is only our ineffectual practice, not the disposition of the malady to go through certain stages, which prevents its being more frequently cured than it generally is. In this, as in every other complaint, no preconceived opinions as to its incurable nature, or its disposition to run a certain course, ought to influence or to paralyze our exertions: it is quite unscientific for a medical man to be thus influenced, and is besides unfeeling, and throws discredit on medicine to allow that nothing can be done.

At the Infirmary for Sick Children, I have employed the treatment now recommended, during the last eight months, in one hundred and eleven cases, independent of many others which I have otherwise had an opportunity of seeing; and I can, without any exaggeration, assert that the application of leeches to the forehead has been followed by almost immediate and very marked success; and I would advise their employment to others, trusting that the practice will be as useful in their hands as it has been in mine. So common is it with me to have recourse to

this remedy, that the mothers, when they bring their children to the institution, have more than once asked me if they should apply leeches or not? and I have met with several instances where the parent told me that she would keep the leeches by her, ready to use, should the little patient again have a relapse, so convinced was she of their utility.

To enter into a detail of cases successfully treated in the manner now mentioned, I conceive to be totally unnecessary; it would only swell this paper to a tiresome length. The particular effect of treatment, or the general observations they suggest, are all that is wanted: the more so, as every man can easily, by his own experience, investigate the truth of what I have advanced, by a reference to the cases which may come under his own observation.

To conclude:—it affords me much satisfaction to be able to add, that my friend, Mr. Webster, of Dulwich, whose assiduity in the pursuit of knowledge and attention to the study of his profession is worthy of praise, has been induced, from observing the success of this treatment at the Infirmary for Sick Children, to give it a trial in his practice; and he informs me that he has done so, in eleven cases, with very decided benefit. In some, the disease was cut short in a week or ten days, which otherwise, in every appearance, would have continued for a month or two. One family, in particular, where three children, of full plethoric habits of body, were severely affected with hooping-cough, showed clearly the utility of this treatment. One of the children, from twelve violent fits of a night, was, two days after the application of these leeches to the forehead, (taking at the same time a purgative mixture,) only five times affected; the leeches were applied a second and a third time, when the boy had only one fit during the night: in a few days afterwards he was free from all complaint. The other two cases did equally well.

October 10th, 1822.

Remarks on the Utility of Cubebs in Chronic Inflammation of Mucous Surfaces. By JOHN FOSBROKE, Esq. Surgeon, Berkely.*

(From the London Medical Repository.)

The case of inflammation of the mucous coat of the intestinal canal, which was treated with cubebs, and some time

* These remarks were contained in a letter to the EDITOR, and were not originally intended for publication. This circumstance must apologize for their brevity. However, as Mr. Fosbroke appears to have been the first who prescribed cubebs in the derangements of the mucous coat of the digestive

afterwards published in the REPOSITORY,* has been succeeded by others in which this remedy was prescribed with uniform success. These cases presented the various species of derangement, from the common symptoms of functional disorder of the digestive tube to sub-acute or chronic inflammation of the villous tunic, seated more or less in particular portions of it, or extending in a greater or less degree along its whole surface.

In a very obstinate case, occurring in a young woman of a cachectic habit, who had twice been reduced nearly to dissolution by chronic inflammation of the villous coat of the intestines, enlargement of the mesenteric glands, and symptomatic cough, with defective menstruation and dysuria, the cubebs were given, combined with bismuth, and leeches were applied to the abdomen: she recovered perfectly under this treatment.

While at Edinburgh, last winter, a case of chronic inflammation of the œsophagus, attended with dysphagia and increasing stricture, came under my care. It had been for about twelve months treated in vain by bleeding, blisters, &c., by individuals of the first and most deserved eminence. It yielded, in a few weeks, to cubebs, combined with the subcarb. of soda, and to the external application of the tartar emetic ointment.

In inflammation of the mucous coat of the intestines, especially when chronic, and in gonorrhœa, where it fails when given alone, I have always succeeded by combining it with Cayenne pepper, or with balsam of copaiva.

June 7, 1822.

Case in which a number of Needles were extracted from various parts of the Body, in a Female. Communicated in a Letter to Dr. GRANVILLE, by Dr. C. OTTO, of Copenhagen.†

(From the London Medical and Physical Journal.)

This paper, which first was read before the Medical Society in Copenhagen, is an account of one of the most interesting cases of the modern time. The patient, about whom it treats, is, first, a proof how much it is possible to suffer; and offers, secondly, a form of disease which certainly belongs to one of

canal, and as our own observations have convinced us of the utility of this substance, under circumstances similar to those instanced by him, we think it due, both to his originality and to the medicinal character of the cubebs, to place these remarks before our readers in the form in which they reached us, several months since, and to call their attention to the properties of this remedy in the disorders alluded to.

* Vol. xvii. p. 458.

† This communication being written in English, we have thought it would add to its interest to leave it with such alterations only as were necessary to make it intelligible.

the rarest. She has long time attracted the whole attention of the public in Copenhagen; and at last the physician, who already is celebrated as a physiologist and author of a paper "De Vita Fœtus," of "Wounds in the Chest," and of "an Examination whether we see with two Eyes or one," has published the case. It seems, indeed, almost impossible; and, although the name of the author would be sufficient, he has however, in order to avoid every suspicion, added the names of a great quantity of medical men who have seen the patient, or have been present at the operation. The case is:—

Rachael Hertz, was first, in the month of August, 1807, affected with a violent colic, which, though several times cured, yet in November was replaced by an erysipelas in the face, that was united with fever, watchfulness, anxiety, disquietude, &c. The physician succeeded in chasing this away; but the fever returned several times, and the erysipelas with it. In April, the patient began to lose her flesh and languish. The face was pale; she had disgust at every food; cardialgia; internal and external heat; all the faculties of mind became weak; she fainted several times in a day, and the whole body trembled so that she could neither stand nor sit. Against this form of hysteria every medicine was tried, but, in fourteen months, without the least success: all the symptoms, often joined to epilepsy, continued with all their force.

At the end of April of the following year, the disease showed itself in another form: to the faintness were added spasmi tonici and clonici, which often were joined with delirium, madness, and even tetanus. This lasted unto the 20th of May in the same year, upon which day a vomitus cruentus arose, by which an immense quantity of coagulated blood was thrown up in three or four days; afterwards all the other symptoms, as faintness, sopor, delirium, cough, singultus, continued to torment the patient, without any relief for seven months. All the physicians despaired now of her life.

In January, 1809, the symptoms changed, but not to the better: to the cough and singultus, painful affections of the stomach, ructationes, vomituritio and tormina, with a fixed pain in the left curvature of colon and perpetual cardialgia, and at last an obstinate retentio urinæ, were united. The cause was undoubtedly a cramp in the intestinum rectum and urethra; and the catheter, clysmata, semicupia, fotus aromatici, &c. prescribed, by which the affections of the bowels were lessened; but the ischuria remained without any relief for two years, and it was necessary every night to evacuate the urine by the employment of the catheter, by which the urethra grew so sensible that the pain was excrutiating, and often excited convulsions, which forced the physician many times to wait.

In the beginning of May, 1809, the hysteric symptoms lost suddenly much of their violence and frequency, but did not go away entirely. This apparent tranquillity did, however, not last long; for soon every sleep was changed to a sopor, which shortly after grew so profound that no degree of excitation was able to awaken the girl. The sopor, having continued any time, ended always with a violent delirium and madness, which, as well as the convulsive fits, lasted until night. For some time the physician succeeded in putting an end to the sopor, by snuff in the nose, and so making the patient quiet; but she became soon accustomed to this means, and the dreadful cramps returned, notwithstanding, at the wonted times.

This state lasted for nineteen months, (that is, from the 18th of May, 1809, unto the 8th of December of the following year,) with uninterrupted force. All medicines were in vain; the patient seemed insensible against all: she got, for an instance, eighteen grains of opium in six hours, without any effect at all, and sixteen grains of tartarus stibiatus in two hours produced neither vomitus, opening of the bowels, nor the increased secretion of urine nor sweat.

At the end of November, 1810, the patient fell into such a languor that she was not able to stand on her feet, but always remained in her bed. The convulsive fits ceased, but the sopor increased so much that she almost slept the whole day and night. On the 8th of December, 1810, symptoms of fever attacked her, with diminution of all the other symptoms. The physicians avoiding to disturb nature in her beneficent operations, a perfect crisis followed on the ninth day, and the girl seemed suddenly to be quite well. For two years the health was good. In April, 1813, she had the measles; and in July a remittent fever, with spasmodic cough and bloody vomitus. Refrigerants put all that to flight, and she was again quite well unto the 13th of January, 1814; but on this day a large carbunculus appeared on the anterior and middle part of the left thigh, which was cured about the 20th of May, 1814. In two years she was again free from every complaint; but then she was affected of a rheumatic fever, with violent pain in the stomach and left hypochondrium, and vomitus cruentus. This state lasted four months, unto September. Afterwards she was well for three years; but the 8th of January, 1819, a new tragedy began. A colic pain tormented the patient, and in a much higher degree as before: she could neither sleep nor touch the belly without the most excrutiating feeling. To this were added a violent fever, singultus, vomitus cruentus, and evacuation of a black and stinking matter through the anus; and in this dreadful state she could bear neither food nor medicines, nor clysmata, nor fomentations of the belly: she grew, there-

fore, quite exhausted, and the end of her sufferings seemed not far.

A more exact examination of the abdomen being made, a hard tumour, divided into three lobules, was felt under the umbilicus. The physician, suspecting an abscess, applied a cataplasma emolliens; but the sufferings continued unto the 12th of the same month (February, 1819); the extremities grew cold; and Professor Herholdt resolved, as the last means, to make an incision into the tumour,—no matter, but a little blood ran out; but, examining the deepness of the wound, he met with a hard and metallic body, and extracted suddenly a common needle. All the symptoms grew milder, but shortly after they increased again; and a point being observed in the left lumbar region, at the touch of which the most acute pains were excited, an incision was also made there, and another black and oxydated needle was extracted. So the pains were renewed, and so he, now here, now there, cut through and found needles; and so, from the 12th of February, 1819, unto the 10th of August, 1820, two hundred and seventy-three needles were extracted! In an annexed table, the different places and the number of needles on each is enumerated; so from the left lumbar region, 39; from the left breast, 22; from epigastrium, 41, &c. &c.

In the intervals the girl was quiet; but, as soon as the needle was near the skin, the most acute pain, with fever, singultus, and vomitus cruentus, was excited. It was often necessary to cut through muscles, and the abdomen swelled very much before the needle could be indicated. By a sudden fright, a palsy of the extremities was added to the sufferings of the unhappy; but, after the 10th of August, 1820, after which no needle showed itself, all the bad symptoms ceased; and, by the use of nervina and roborantia, warm baths, vesicatoria, linimenta, the feeling by and by returned, and she seemed in short time quite cured.

In the hope, now, that all was over, the author read this case before the Royal Medical Society; but, since May, 1821, the girl suffers of the most obstinate retentio urinæ, after having had a sort of diabetes. What end this takes, the author will publish in the future.

Copenhagen, August 18th, 1822.

Cases and Observations on Simple Chronic Inflammation of the Uterus, in which State this Organ may become Retroverted.

By John Robertson, M. D. Glasgow.

(From the Edinburgh Medical and Surgical Journal.)

The great difficulty in comprehending the real nature of many uterine disorders, and the frequent want of satisfactory infor-

mation from reading on such subjects, have induced me to beg the favour of your inserting the following observations in your useful and interesting Journal. They formed the subject of a paper read to the Glasgow Medical Society in November, 1817; and the occurrence of several cases since, lead me to attempt a general explanation of the Nature, Diagnosis, and Treatment of the affection in question.

The first case I propose noticing, was that of a carter's wife, a delicate looking woman about 30 years of age, who, during three months, had observed no menstrual discharge, and had since been troubled with flatulency and indigestion, and triflingly affected with what she called a gravellish complaint. The latter had recently become rather more troublesome, attended with a weakness of her back, and slight uneasiness in her groins; yet of so little consequence did she deem her complaints, that she applied no remedy, and went about her daily occupation as usual.

On the 27th November, 1816, however, while walking across the floor, she was suddenly and unaccountably seized with excruciating pain in the lower part of her belly. Immediately thereafter I was sent for, and found her shrieking from agony, every few minutes aggravated by violent spasm, almost exclusively referred to the umbilical and lumbar regions. Her countenance was keenly, or rather wildly, anxious; she lay in a half recumbent posture, with the knees drawn up towards the belly; her tongue was dry and parched; heat of skin rather increased; the pulse beat so quick and small that it was in vain to reckon it; and, on inquiry, she denied any suspicion of her being pregnant.

About ℥xvi. of blood were drawn from the arm, when she sickened and became faint. The pain abated; but her strength reviving, it threatened to return, when a tea-spoonful of laudanum was exhibited, which, in a few minutes, losing its effect, was repeated. Fomentations were then applied to the abdomen; an anodyne enema was administered; and, in an hour, I left her drowsy, and nearly free of pain.

On the 28th, I found her comparatively easy; complaining but of a sense of weight and soreness of the abdomen, especially the lower part, and also of the back and thighs; she was harassed with a continual desire to void both her urine and feces; and, on attempting to walk or stand, she experienced the greatest pain. She still denied being pregnant; and therefore was averse to examination *per vaginam*. The horizontal posture was enjoined, and castor oil frequently exhibited.

She remained more or less in this state till the 5th of December, when the pain, in all its violence, recurred, accom-

panied with a distressing flatulent distention of the abdomen. The above practice again was had recourse to, and was followed by nearly a similar result, the flatulent tendency being afterwards overcome by small and repeated doses of calomel, magnesia, and rhubarb.

During this attack, I remarked that, while the spasms were less frequent, they were attended with a more decided bearing down than before; that, on remission, the pain confined to the lumbar and hypogastric regions was dull, heavy and constant; that the dysuria and tenesmus were still distressing: and she acknowledged that, during the last month, she did occasionally feel shooting pains in the mammæ.

Impressed with these circumstances, I insisted on examination, and found the uterus considerably lower than in the natural unimpregnated state; enlarged to what is common about the fourth month of pregnancy; but so tender to the touch that little freedom could be used. She felt weak; and her pulse, though quick, was rather feeble.

Did such a state warrant the suspicion of pregnancy? I thought so—said so—and predicted to the friends, that should another attack occur, abortion most likely would be the consequence. Some source of unusual irritation, however, evidently existed in the uterine region; she was troubled with a frequent painful cough; and her pulse was quick and irritated. On which accounts, a large blister was applied to the hypogastric region, and small doses of tartar emetic exhibited.

By these means, she became greatly relieved, the pain being nearly subdued; the cough had entirely ceased; and the pulse greatly more calmed. But on the 14th, she complained of a trifling relapse of the pain, which was removed by fomentation and the opiate liniment with ammonia. On examination *per vaginam* at this time, the uterus appeared to me less dependent; not nearly so tender to the touch; and, as I fancied, greatly more buoyant then when the symptoms were more urgent. So that these circumstances seeming to support my belief in her pregnancy, together with the fact of the uterus being felt enlarged in the hypogastric region, and the supposition of its having, as usual, risen above the brim of the pelvis, I rather too rashly prophesied that no return of the pain would take place, and that it was not likely abortion would occur. On the 16th, I observe her reported in my notes free from pain, but still extremely weak.

Thus she continued for about fifteen days; when, on the 2d of January, she again was attacked with the same symptoms as above. On examination, I found the uterus descended lower than when I last examined; immovably impacted in the cavity

of the pelvis; the fundus turned backwards; and the *os uteri* tilted forwards behind the upper part of the arch of the pubes.

I now looked on the case, as one of *retroversio uteri*, and, accordingly, endeavoured to reduce the malposition, but ineffectually. The attempt was attended with considerable pain; and, neither the tenesmus nor dysuria being at all urgent, I deemed farther efforts unadvisable. But recollecting the advantage formerly derived from the blister and tartrite of antimony, I repeated them with evident advantage, although less so than before; for the pain, in three days after, returned with a severity at least equal, requiring the large exhibition of opium *per os* as well as *anum*, thus for the time diminishing the patient's torture.

At a loss what to do—the misplacement remaining, still seemed to threaten a relapse; partly therefore at random, and partly with the hope of reducing the irritation, I recommended a pill, composed of gr. ij. of calomel and gr. i. of opium, to be taken three times a day. From the commencement of these, no violent pain ever after occurred. In three days the mouth became slightly affected, when the pills were omitted. The uterine tumour, although not obviously diminished, became much less tender to the touch, and a bloody serous discharge (not inaptly likened by the patient to "moss water") began to ooze from the uterus.

The symptoms, nevertheless, were still sufficiently urgent; when, about this time (the middle of January), Dr. Baird saw her with me. He agreed with me in thinking the uterus retroverted; and ineffectual attempts were again made to reduce it. He suggested the possibility of very copious injections *per anum* assisting the efforts to raise the fundus; but all to no purpose.

A few days after, the above pills were again had recourse to, but less frequently taken; castor oil was occasionally exhibited; and small doses of cream of tartar and nitre given as a diuretic. The bulk of the womb soon began, very slowly, but evidently, to diminish; the irritation on the rectum and bladder in proportion abated; but the discharge alluded to, for a considerable time, continued in varied quantity: for which, she was recommended to use injections of warm water, infusion of chamomile, and so forth, dreading lest more astringent injections might reproduce the pain.

Finally, although I long continued to think pregnancy to have been connected with her complaint; yet, to this day, there has never appeared any thing like the blighted remains of a fœtus. I saw her in March 1817 in good health; free from discharge; the uterus, to the touch, in every respect natural; and I then

learnt from her husband, that the first symptom of her complaints was an occasional *dolor internus ex coitu.* Within these few days I have heard of her since continuing well.

The above interesting case, it will readily be observed, was far from understood; and the impression left on my mind was far from satisfactory, till I had occasion to contrast it with the ollowing.

On the 17th of June, 1817, I was requested to see a girl, 19 years of age, who, by the neighbours, was considered dying. On entering the house, I found her resting on her knees and elbows, weeping like a child from the severity of her pain. I was informed, that, ten weeks previously, she had been delivered of a still-born child of the full time; that her labour was natural, and her recovery so good, that on the 10th day from the period of her delivery, she resumed her work in a cotton-mill. I was most minute in my inquiries respecting the extent of her recovery, particularly regarding any tumour or fulness in the lower part of her belly, a degree of which she acknowledged was perceptible for the first three weeks; but after this, to use her own expression, she "felt as swamp as ever." I learnt, however, that the lochial discharge had ceased unusually soon; and that, although she could pass her water freely enough, she had more frequent calls to do so than before.

Such was her state, till within two days of the time I saw her, when she observed, that without any obvious cause, her calls to pass urine became still more frequent; feeling at the same time a kind of dull uneasy weight in the loins, with a sense of trifling pain in both groins; yet of so little consequence did she esteem these, that her relations were not informed of her ailments, nor did she quit her work till the forenoon of the day on which I saw her.

Her pulse was quick, feeble, and even tremulous; the pain, judging from appearances, was truly agonizing, chiefly referred to the umbilical and hypogastric regions, although the whole abdomen, back and thighs, were also complained of. The urine and feces were reported to have been very lately and freely evacuated; the menstrual discharge had not appeared since delivery; but she being liable to nervous affections, and of late much troubled with flatus, I was inclined to coincide with the old women's decision, of my patient's complaint being violent colic. With this view, I dropped her out 60 drops of laudanum; and farther prescribed a dose of castor oil and calomel, to be taken an hour after. In the evening I found her much relieved, and asleep.

But the morning following, I was disappointed to find her as ill as ever; principally complaining of the regions of her pubes, back and groins; of frequent inclination to pass water, and a

constant desire to evacuate *per anum*, which, when attempted, was accompanied with such a strong and involuntary bearing down, as if, she said, her "whole guts were coming away."

Struck with the suddenness of the attack, and the strong resemblance of the symptoms to those of the last case, I immediately examined *per vaginam*. I then discovered the uterus considerably lower than it ought to have been; enlarged, as nearly as I could guess, to about the bulk of one's fist; the fundus being directed backwards into the hollow of the sacrum; while the mouth, which was tumid and much indurated, was felt facing the lower edge of the symphysis pubis. The whole body of the womb was so tender, that she instantly screamed from the most trifling pressure on it; and when she attempted to walk, sit, or stand, she felt the greatest difficulty and pain. Her pulse was quick, small, more feeble than hard; and, during the night, she had several times been attacked with severe vomiting.

Being satisfied that neither urine nor feces were retained, for as to the former the catheter was introduced, my first object was to guard against inflammation. But ere ℥viii. of blood were drawn from the arm, my patient fainting, I was obliged to desist. I then endeavoured to allay the pain by fomentation and opium. With the view of reducing the morbid bulk of the uterus, I had recourse to the pills already mentioned, composed of calomel and opium. As she felt easiest when lying on her belly, or leaning on her knees and elbows, I strictly enjoined these postures, and forbid every thing but gruel and panado.

I left her with the expectation of her soon becoming relieved; and, the day following, was glad to find her comparatively easy. The uterus, nevertheless, remained *retroverted;* and, although less tender to the touch, was not perceptibly diminished in size. The irritation on both rectum and bladder were still distressing; and the *os uteri* felt so hard, that Dr. King, who this day saw her with me, was inclined to suspect, notwithstanding the girl's youth, the presence of scirrhus. He judiciously suggested the application of leeches to the external parts, and approved of the continuance of the calomel and opium. Ten leeches were applied, some within, and all in the neighbourhood of the vulva; the bleeding was full, and kept up for several hours; but alteration in the size of the uterus did not immediately become apparent, although it could be pressed on with considerable freedom.

On the 4th day from the commencement of the pills, the gums became slightly tender. The uterus still retained its malposition; yet it felt somewhat flaccid, and perhaps might have been replaced. The tenesmus and dysuria, in two days

more, had almost subsided; and so well did she feel herself, that it required no small persuasion to dissuade her from leaving bed. The pills, at proper intervals, were continued so as to keep up a trifling irritation on the gums. And on the 16th day from the time I saw her, the uterus, although perhaps a little heavier, felt nearly as free and buoyant as in the healthy unimpregnated state. No discharge was at any time perceptible.

These cases have brought to my recollection a third, which I imagine to have been similar; although, at the time, certainly not understood. Having no notes of the case, I can only vouch for its general features.

A woman, of a feeble appearance, and about 50 years of age, laboured under general pain of abdomen, hiccup, vomiting, and a most distressing tenesmus, with inability to pass any thing but a little mucus. Her pulse was quick and feeble; her belly was considerably distended; but no particular urinary complaint. By account, her complaints had come on suddenly, had been present for several days, and had become gradually more extensive, in point of pain of abdomen, which at first was confined to the lower part.

The case was considered as one of constipation; and purgative after purgative was accordingly exhibited. They seemed to operate tolerably well, but all to no purpose; the symptoms continued urgent; severe and frequent was the unavailing tenesmus; and yet the *imagined* collection of feces could not be expelled.

The rectum was ordered to be examined. This was done by another gentleman and myself. The gut was found empty; but encroached on by a tumour, which, *per vaginam*, was discovered to be the uterus, so tender to the touch that the woman complained grievously on its being pressed.

We were then, it must be acknowledged, but novices in the touch so necessary in such cases; yet conceited enough to solve the state to our own satisfaction. We never thought of questioning the above view of the case; and we readily supposed that accumulation of feces, with the strong and repeated bearing down present, would easily produce a falling down and tenderness of the womb!

On mentioning the *procidentia* to the physician, it was deemed immaterial. The patient, however, gradually sunk; and, on the 4th or 5th day from her admission into the hospital, was cut off by that insidious enteritic affection, which, I fear, will generally be found to baffle our utmost efforts.

Inspection exhibited the intestines generally inflamed; no where adherent to each other; but covered, in many places, by a shreddy coating of lymphous exudation. No feces were

found in the intestines, as was predicted, but plenty of wind; and, to our surprise, no uterine tumour was found to encroach on the rectum; although we did remark the womb to be rather fuller, softer, and darker-coloured, than was common. I may add, that I distinctly recollect the tenesmus to have ceased fully 30 hours before the woman's decease.

OBSERVATIONS.

1. *Nature of the Disease.*—The occurrence of retroversion, I consider, is to be looked on as an accident, superadded to an altered state of the uterus previously existing; yet so suddenly and so alarmingly did this accident change the features of the complaint, that it required the most decided notice, and demanded the promptest assistance.

I am anatomist enough to know, that, in the healthy unimpregnated state, *retroversio uteri* cannot possibly take place; or, if it can, it certainly never could be productive of the distressing symptoms detailed in the two first cases. We know it occasionally to happen in early pregnancy. It has been reported, too, sometimes to take place in enlarged scirrhus uteri; also to accompany some states of diseased ovarium; and, in Mr. Astley Cooper's Treatise upon Hernia, notice is taken of a fatal case which occurred to Dr. Marcet, where, for six weeks, vomiting and constipation were the prominent symptoms; and, on inspection, the uterus was found retroverted without pregnancy —the fundus being turned into the space between the rectum and vagina.

In the cases before us, (I allude but to the two first,) that retroversion did exist, will not be denied; and there is proof sufficient, that no malignant disease affected the uterus of either. Further, that the uterus was enlarged, there can, I think, be as little doubt; and if so, could it be from any thing but simple chronic inflammation? In the first case, that disease and enlargement subsisted in the uterus, long before the retroversion took place, appears from the absence of the menses, the irritation of the bladder, the weakness of the back, and the uneasiness in the groins, which were present fully three months before the accident occurred. In the second case, from the lochial discharge having so prematurely ceased, the probability is that the uterus had never, since delivery, become fully reduced. The frequent inclination to pass water seemed proof of this; and this increasing, together with the uneasiness in the back and groins, afford at least presumption of preternatural irritation going on.

In such a state, then, do I believe the uterus of both patients to have been. Their rank in life requiring daily exertions, would not only prevent the abatement of the disease, but very

likely assist in its progress. Accidents of various kinds might force the fundus below the promontory of the sacrum, from under which it could not be supposed easily to extricate itself, especially as the dysuria and tenesmus, instantly produced, inevitably must have excited that involuntary bearing down, which would not merely increase the malposition of the uterus, but, as it were, impact it into the cavity of the pelvis.

According to this view of the case, the uterus was already inflamed, or, if not strictly so, in such a state as to render the accession of inflammation both easy and almost instantaneous, from the new, and, as it were, strangulated position it had acquired, this position acting as a powerful source of mechanical irritation upon an organ already under morbid excitement; so that neither the suddenness nor violence of the attack seem incompatible with the *natura morbi* I argue. The extreme tenderness of the uterus, and the quickness of the pulse, seem to have arisen from the inflamed and irritated state of the former; while the tenesmus, dysuria, pain of back, groins and thighs, appear attributable to the mechanical irritation on the parts contiguous, from the womb's morbid bulk and malposition. As to the analogy the third case is supposed to bear to the two first, I would remark, that the suddenness of the attack,—the pain of abdomen, at first confined to the lower part,—the vomiting,—the severe and frequent tenesmus,—the rectum being encroached on by a tumour, which, *per vaginam*, was discovered to be the misplaced uterus so remarkably tender to the touch, are all circumstances at least strikingly suspicious. But what, it will be asked, had become of this uterine tumour on inspection? This, however, in place of being subversive, is the very fact I consider the case chiefly valuable for. Of its existence during life, we were positively certain; but the inflammatory action on the peritoneum having become more general, the same degree still existing in the uterus, was probably incompatible with the delicate state of the woman's constitution. Resolution therefore in part took place, permitting, as was noticed, the tenesmus, fully thirty hours before death, to cease; and, on inspection, exhibiting the womb only rather fuller, softer, and darker coloured, than was common.

Such cases are certainly rare. My reading is not so extensive as to say where similar are to be found; but it strikes me I am acquainted with a species of uterine disorder not unlike the state described in the two first cases previous to the occurrence of retroversion, meriting serious attention on account of its own importance; and, if my suspicions be correct, the cases above narrated will teach us, in a prophylactic point of view, to be especially active.

The *obliquitas*, the *dislocatio*, the *procidentia*, and the *dolor uteri*, seem all modifications or degrees of the same complaint; some arising from pregnancy, some from malignant, and others, as in the present, from benign enlargement of the uterus.

I have frequently known cases about the age of puberty, but especially in prostitutes and married women, where, besides symptoms of general irritation, an uneasiness or pain in the back, lower belly, and thighs, a frequent inclination to pass water, with a straining on doing so, and sometimes a difficulty of walking, suddenly sitting down, or rising up, have been complained of. In some, these symptoms are accompanied with a transparent and colourless, and in others with a yellow-coloured, mucous discharge from the vagina; but seldom without any. I have stated such especially to occur in prostitutes and married women. I will not take it upon me to say what the exciting cause may be; yet I am satisfied that, in such cases, a tenderness of the womb does in certain circumstances exist more or less. The above set of symptoms, moreover, are not unfrequently met with in women who have risen too soon after delivery; and it is an observation as old as the days of Hippocrates, that one cause of a falling down of the womb is, "*coitus brevi post partum.*"

It was but of late I learned to appreciate justly the nature of these symptoms; and, from experience, I have no hesitation in stating, that in the majority of such cases, the uterus will be found more or less preternaturally dependant; the cervix, to the feel, somewhat elongated; the *os tincæ* more contracted than in health; and, in general, pressure will excite pain. A person requested me to visit his friend, who laboured under pain in her loins, lower belly, groins, and thighs, dysuria, and a disrelish for her trade, from the pain she experienced; had no appetite for her food, and she was distinctly feverish. On examination, I found the womb, as nearly as I can describe, as above. But, in a few days, a small bleeding, strict confinement, a few doses of calomel and opium, with the hip-bath, completely established her recovery. Two years ago I was consulted by a respectably married woman, who for nine months had more or less complained of symptoms similar to the above; with the addition of a difficulty of walking, or suddenly sitting down. She had used many remedies with but temporary benefit. From my questions a good deal coinciding with her feelings, I was readily permitted to examine *per vaginam;* when the uterus was felt distinctly heavier, both tender and depressed. The same treatment as above was had recourse to; the calomel and opium longer persisted in; and the husband seriously advised to keep apart. In about three weeks her complaint left her, and has never since returned.

Such, then, seems to be the nature of the affection alluded to. As to a name,—if that irritation of the bladder, approaching to slight inflammation, and attended with increased mucous excretion, be entitled to the appellation of *Catarrhus Vesicæ*, why should *Catarrhus Uteri* be denied to this?

As causes—Puberty, recent delivery, period of menstruation, and fluor albus, may be considered predisponent; and inconsistent exercise, excess of venery, cold, all sources of local irritation, and perhaps sudden cessation, of the menses and fluor albus, as exciting.

2. *Diagnosis.*—What has already been remarked, may in a great measure supersede detail on this point.

First, It may be confounded with pregnancy. The uneasiness of back, belly, and thighs, the dysuria, and painful walking, may be common to both; but, in the latter, there is generally no discharge, and seldom the uterine pain and tenderness alluded to. The uterus, in both, may have descended somewhat; yet, in pregnancy, the os uteri is turned more backwards, and both it and the cervix for a considerable time feel natural. In very early pregnancy, the distinction will very difficultly be decided on; but fortunately in both, the same treatment will equally avail.

Secondly, It may be confounded with more serious and intractable states of the uterus, as scirrhous and other malignant affections. The progress of these is more slow; the pain is not dull and constant, but shooting, occasional, and severe; the *os uteri* is generally more open, and often apt to bleed on trifling freedom being used; the time of life will assist; and the colour and expression of the countenance are too often indicative of the nature of the disease.

Finally, the state of the uterus will distinguish it from all abdominal and bladder affections. Where retroversion has occurred, proper attention to the malposition and extreme tenderness of the womb will sufficiently characterize it from threatened abortion.

3. *Treatment.*—From my observations on the nature of the disease in question, three indications would seem readily suggested.

First, To arrest the inflammatory action by low diet, evacuants and bleeding.

Secondly, To allay the irritation by fomentation and opium.

Thirdly, To reduce the morbid bulk of the uterus by blisters, stimulating embrocations, strict confinement, and mercury.

Should the symptoms be trifling, a little confinement to the horizontal posture, spare diet, laxity of the bowels, the hip-bath, and, above all, abstinence from venery, will generally soon effect a cure. But should the symptoms be more urgent, espe-

cially if attended with fever, in addition to the above, bleeding, either locally or generally, must not be omitted. Again, should the slightest tendency to, or actual retroversion have occurred, we have to consider the case of a most serious nature; we have not only to remove the local inflammation, but to guard against its extension to the bowels. Yet the pulse seems to counterindicate the lancet! But why do we ever bleed in inflammatory affections of the stomach and bowels? It is because we know these organs to be so essential to the well-being of life; that when, by inflammation, their functions are impaired, health or the general constitution does not only remarkably suffer, but there seems, as it were, such a tendency to speedy dissolution, that the depressing cause must instantly be removed. And do we not likewise know that there are other organs, which, although not so immediately connected with life, are exceedingly liable to sympathise with those parts which are? Such are the lungs, the testes, and, I would add, the uterus, which, when diseased, very soon, or at times, immediately acquire a similarity in symptoms, especially in the quickness and lowness of the pulse. With the same propriety, therefore, I would urge, that in the present case, blood should be drawn, according to the duration of the complaint, and state of the patient's constitution —bating, of course, such a lowness of the pulse as may be indicative of death.

As to reducing the malposition by manual assistance, I feel satisfied, that, in cases so acute as those recited, it will generally be found impracticable. Nay, from the extreme sensibility of the uterus, such attempts may prove seriously hurtful.

Purgatives, when the symptoms are violent, I should think generally injurious, as tending to increase the tenesmus and bearing down; but where they are indispensable, small and repeated doses of the neutral salts, castor oil, and injections, as being less irritating, should alone be employed.

The hip-bath, in the form of heated water or vapour; the free use of opium, especially *per anum* or *vaginam*, will be found strikingly serviceable in allaying the local irritation.

With the view of preventing a relapse, by reducing the morbid bulk of the uterus, confinement to the horizontal posture must be strictly enjoined, and perseveringly continued, according to the obstinacy of the symptoms. But for this purpose, mercury must especially be relied on. In the form of calomel, it is perhaps the most convenient and speedy in its effects; and, by conjoining it with opium, the constitutional and local irritation are not only quieted, but (to speak figuratively) the bowels protected from the acrimony of the metal—a matter in this case of no small moment. Slight mercurial irritation is all that is desirable. As soon, therefore, as it appears, the medicine should

be omitted, and small doses of neutral salts frequently given to prevent salivation. It is again, however, to be had recourse to and persisted in, till no indications for its continuance remain, which, in some more chronic cases, will not occur till the lapse of several weeks.

Of the utility of diuretics, I am doubtful; and unless the bladder participate in the neighbouring irritation, becoming painfully sensible to the acrimony of the urine, hence requiring this secretion to be rendered bland and inoffensive, I am rather of opinion the more this organ be kept at rest the better.

Glasgow, February, 1822.

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*On the Pathological Anatomy of the Human Brain and its Membranes.** By DAVID CRAIGIE, M.D., Lecturer on Anatomy and on Physiology, Edinburgh.

(From the Edinburgh Medical and Surgical Journal.)

"Quinetiam morbis in corporis avius errat
Sæpe animus; dementit enim, deliraque fatur
Interdúmque gravi lethargo fertur in altum
Æternúmque soporem, oculis, nutúque cadenti:
Unde neque exaudit voces, neque noscere vultus
Illorum potis est, ad vitam qui revocantes
Circumstant, lacrymis rorantes ora, genásque.
* * * * * * * *
Quinetiam, subitâ vi morbi sæpe coactus
Ante oculos aliquis nostros, ut fulminis ictu,
Concidit, et spumas agit; ingemit, et tremit artus;
Desipit, extentat nervos, torquetur, anhelat
Inconstanter; et in jactando membra fatigat."

Lucret. de Rerum Natura, lib. 3.

The anatomical relations of the organ contained in the cranial cavity are somewhat complicated, and, when rendered

* This paper was written nearly two years ago, and read to the Medical Society of this place in the Session 1820–21; and was then intended as a mere outline of a more complete Essay on the subject. As the engagements of the author will not allow him to finish it in the manner in which he originally intended, at least within an early period; and as he is unwilling to treasure it to the ninth anniversary, with the vain hope of rendering it more worthy of the public eye, he has resolved to let it appear with whatever errors or defects may cling to it. To those who have read the interesting and elaborate letters of M. Lallemand, it may be requisite to say, that in some points, a coincidence of opinion on the morbid change which has been recently described by the French pathologists, under the name of *Ramollissement du Cerveau*, will be recognised. But this coincidence is purely accidental; and that no reason may be assigned for ascribing it to any other cause, the Essay is printed, with the exception of a single paragraph, *verbatim* from the records of the Society to which it was read. It may be remarked, that the papers of M. Rostan, which were originally published in the Nouveau Journal de Medecine, do not exhibit those points of similarity which would render any explanation like this necessary. The author, under these circumstances, thinks it would be mere affectation to avoid referring in notes to those parts of M. L.'s Treatise in which the coincidence is most conspicuous.—D. C.

subservient to Pathology, they cannot be said to increase in simplicity. This circumstance cannot be attributed to deficiency of investigation, or a narrow and limited collection of the effects of morbid agents; for almost no organ has occupied a greater proportion of the attention of professed anatomical inquirers, while the effects which its morbid states necessarily produce on the system, have rendered it an endless subject of examination to the pathographical physician. We are, indeed, in possession of numerous observations and records of the various pathological conditions in which almost every part of the human brain has been found; but it is rather discouraging to remark, that few general principles have been educed from them, and that we meet with great discrepancy and discordance in tracing the history of the effects of the same organic lesion. It is impossible for us, in the present Essay, to aspire to the difficult task of reconciling those differences, of arranging isolated facts under general heads, or of explaining and correcting the mistakes of former observers; but we propose to exhibit a brief and rapid sketch of the most important deviations from healthy structure, which have been observed in the human brain and its appendages, and to connect these, as far as may be practicable, with the effects which they produce on other organs and different functions. In performing this duty, we deem it by no means necessary to describe the healthy structure of the organ; but if we shall be occasionally led to make any passing remarks on this subject, we trust that they will be received as what personal observation has shown us to be indissolubly connected with the morbid changes, and indispensably requisite in explaining them.

The whole subject embraces two general divisions. I. The morbid changes of the brain and its appendages. II. The connexion between these changes, and others which exist in different organs.

The former of these divisions I shall examine under the subdivisions of, 1*st*, Morbid changes in the membranes; 2*d*, Morbid changes in the convoluted surface; 3*d*, Morbid changes in the central surface; 4*th*, Morbid changes in the substance of the encephalic mass.

The dura mater, the first and strongest membrane of the brain, may be considered as having a threefold use; 1*st*, That of an interior periosteum; 2*d*, That of a protecting membrane; 3*d*, That of influencing the circulation, by its sustaining the large venous trunks. It performs the part of a periosteal covering to the internal table of the cranium, to which it is connected by numerous arteries and veins, with a degree of firmness which is in general considerable, but is liable to great variation. These vessels by which it is indirectly connected with

the pericranium, seem, in some instances, to be capable of a morbid action, in consequence of injuries of the exterior envelope. This sympathetic action, as it has been named, has been fully considered, and very much overrated by Mr. Pott, who has laboured to impress the profession with the notion, that the dura mater is always in an inflamed and subsequently sloughing condition when the pericranium has been severely injured. There is, however, no direct proof that this is a uniform result. That it has existed in some few instances, we are perfectly willing to admit; but I am satisfied, from many observations on this point, that it takes place much less frequently than has been supposed. I have seen the pericranium detached for the space of two square inches, and a granulating process commence from the external table, but without the slightest proof of the dura mater being diseased. In the young subject, it is by no means uncommon, when the pericranium is injured, to see the whole thickness of the cranium removed at the spot by the ulcerative absorption, and the wound begin to show the pulsating motion of the brain, and expel purulent matter at every motion of respiration; yet this morbid action will gradually disappear, and leave no trace of any great or important change in the dura mater having taken place. The effects, in short, which injuries of the pericranium produce on the vascular system of the dura mater, are very much varied by the age, disposition, and habits of the individual, and, above all, by the treatment which is employed after the injury.

I have said that the dura mater, in the healthy state, adheres very firmly to the internal cranial surface; and it has been remarked by Dr. Baillie and others, that the firmness with which it adheres may sometimes be so great as to amount to disease. I regret that observation forbids me to assent to this opinion; and I can see no reason why any degree of firmness with which it might adhere should be viewed as morbid. I am, on the contrary, inclined to think, that the most frequent and ordinary effect of disease is to make it adhere with less firmness than usual; and I have, in the course of morbid investigations, met with at least one decided instance in which this appearance was remarkably distinct. A man of about thirty-five years, who had received a slight blow on the head, began, some time after, to labour under pain of the whole syncipital region; and, in the course of a few months, had complete amaurosis of both eyes. Remedial treatment was employed under eminent surgeons without much benefit; and, in addition to his other maladies, he soon became epileptic. After suffering, in a hopeless manner, under his complaints for the space of six months, he fell into a comatose state, and died in a few days. Dr. Duncan junior and myself examined the body in the presence of some

medical friends. On opening the head, we found that the internal table of the cranium did not adhere with the usual firmness to the dura mater, but instantly dropped off as soon as it was divided all round by the saw. The skull-cap, which was thus removed, was unusually thin through the greatest part of its extent; and this thinness was obviously produced by the removal of a part of the osseous structure of the internal table by absorption perhaps. In some few points, the original thickness of the cranium was left unchanged, so as to form the spicula of which pathographical writers have spoken in describing the appearances of the epileptic cranium. These spicula, however, were not produced, as has been generally supposed, by an augmentation of the ossific action, but, as I have already hinted, by the removal by absorption of the bone of the contiguous parts. No blood-drops appeared either on the external surface of the dura mater, or on the internal surface of the cranium; so that the usual arterial connexions of those parts were no longer present. The lateral and inferior regions of the cranium presented the same appearances; and the dura mater seemed to lie very loosely on all these parts of the basis of the cranium, to which it generally adheres with singular firmness. The other morbid changes were such as do not illustrate this state of the vascular connexions of the dura mater and cranium; but I may have occasion to allude to them when speaking of other pathological alterations in this region of the body.*

The dura mater has been described as a frequent seat of hard bodies of an irregular figure, to which the general name of tumours has been applied. I have almost nothing to add to what is generally known on this subject, and must, on that account, say little or nothing regarding them. The osseous degeneration, which is sufficiently common, I have witnessed in the falx; and I have no doubt that it occurs in many other parts; but whether its development depends on an action in the adherent, or the non-adherent surface, I am quite unable to determine.

That the dura mater performs the part of a protecting membrane to the encephalic mass, we conceive to be shown by the productions which it furnishes between the divisions of this organ, and by the peculiar appearance of its interior or non-adherent surface. This appearance has been ascribed to the reflection of the arachnoid membrane,† and has been believed to be established by a great variety of proofs;‡ but whether we

* This case has been since described more fully in the 17th volume of this Journal, by Dr. Duncan, p. 322. I have described from short notes merely what answered my own views.—D. C.

† Bichat Anatomie Descriptive, Tome 3me, pp. 12, 13, &c.—20. § II. Surface cerebrale.

‡ Idem. Tome 3me, pp. 30, 31, 32. § IV. de l'Arachnoide.

view it as an actual reflection, or merely a modification of the surface of the dura mater, will not perhaps have much influence on the opinion which we have now stated. It is obvious, that the interior or non-adherent surface closely resembles that of the arachnoid membrane to which it is applied, and that it is often the seat of similar morbid actions. We shall therefore notice the pathological relations of both at the same time.

The appearance of the healthy arachnoid membrane is abundantly characteristic;—smooth, shining, and almost transparent, it has been generally considered as a serous membrane, although it might be preferable, in the present state of our knowledge, to view it as one of a peculiar nature. It is very probably the occasional seat of morbid action; and we have frequent opportunities of observing very striking deviations from the usual appearance which it exhibits in health. But it is not unimportant to remark, that these alterations depend, in the majority of instances, on a morbid action going on in a different membrane, and in a different region, sometimes of the encephalic mass. It is, on this account, exceedingly difficult to separate the pathological states of the arachnoid membrane from those of the pia mater, or even of some other parts of the brain, either in practical observation, or in nosographical arrangement; and if, in the present instance, we blend somewhat the appearances of these two membranes, we trust that this will be ascribed to the impracticability, and the consequent impropriety of separating them. We think that the necessity of these remarks will be perceived by the descriptive details which are immediately to follow.

The most usual morbid alteration to which the arachnoid membrane is liable, is the diminution of its transparency, without any change in its shining aspect. This is most remarkable in those parts of the membrane which correspond to the cerebral anfractuosities, where the arachnoid membrane has then a tinge of a bluish grey colour; but it is likewise sufficiently obvious on the apices of the convolutions, where it prevents the observer from perceiving the natural colour of these objects. The superior part of the hemispheres on each side of the median fossa is sometimes wholly occupied with this appearance; but when it is present, it is always most conspicuous at the basis of the brain. In such circumstances, the arachnoid membrane is elevated and detached from the pia mater by the interposition of a watery fluid; its entire non-adherent surface has a gelatinous appearance; and some observers have thought that a semi-fluid substance resembling jelly was deposited beneath it; but if a puncture or two be made, a considerable quantity of watery fluid is observed to escape; and the observation of this pheno-

menon is, in truth, a very good method of proving the existence of the arachnoid membrane.

In this instance, it may very justly be made the subject of inquiry, whether the fluid be derived from the external and adherent surface of the arachnoid membrane, or from the external surface of the pia mater. I am uncertain whether this has ever occupied the reflections of those who have had occasion to remark the appearance, and it would not, perhaps, be easy to devise expedients by which the question might be determined. If, however, we admit this fluid to derive its origin from the arachnoid membrane, the opinion must be held as a proof of the inconsistency of Bichat's reasoning concerning the nature of the arachnoid. Every effusion, whether inflammatory or serous, proceeding from a transparent membrane, takes place, we know, from the general characters of such membranes, at the internal or non-adherent surface of these organs. In this instance, however, the effusion takes place at an adherent or exterior surface of a membrane, which, he has laboured to show, possesses the characters of the transparent or serous membranes. We are therefore reduced to the alternative, of either denying the arachnoid origin of the effusion, or of arranging this membrane in a different or individual class. If we still continue to consider the arachnoid membrane as analogous to the transparent envelopes in other regions of the body, we may then believe that the effusion is always induced by some peculiar action in the vascular system of the pia mater. Other reasons, however, influence me still more strongly in entertaining this opinion, on the origin of the effusion beneath the arachnoid. Not only do we generally find the vessels of the pia mater more numerous and more distended in such circumstances, but we frequently, nay, almost uniformly, observe an analogous change in a part which is a continuation and modification of the pia mater. I allude to the choroid plexus, which I have almost invariably, in such an event, found to exhibit in both ventricles a series of bodies resembling hydatids, about the size of a garden pea. Although these have been considered hydatids by many pathologists, yet I have very strong and satisfactory reasons for believing, that they are the result merely of the natural structure of the vascular network being unfolded and distended by the effusion of a fluid, which is produced by the same action going on in a different part of the same membrane. At the posterior extremity of the mesolobe,* in the fissure between the posterior lobes of the brain proper, and the cerebellum, the pia mater may be traced below the fornix into the middle or

* This name given by Chaussier I prefer to the inapplicable circumlocution of *corpus callosum*.

third ventricle, in the form of the *velum interpositum* or choroid web, and subsequently into the lateral ventricles in the form of the plexus choroides. When, therefore, from any cause, a morbid action is commenced, either on any part of the pia mater investing the convolutions, or on that which is found in the central surface, this action may be propagated by continuity of surface from the one to the other, or *vice versa*. It is on this account, I conceive, that we frequently find the effusion between the arachnoid and the pia mater, accompanied by the distended state of the choroid cells, and even a quantity of fluid in the ventricles.

The latter circumstance, which very frequently attends the hydatoid appearance of the plexus, has led some practitioners to consider this as the acute hydrencephalus. It is hardly possible, however, to make a greater mistake in the pathology of these parts, than by confounding the two diseases. The effusion of which we have been speaking, occurs in many subjects in whom there is but a very moderate affection of the head; and, at all events, very different symptoms from those which attend the acute hydrencephalus. When the system is under the influence of common contagious fever, it is exceedingly common to find this change in the head after death; and we generally observe, that the last hours of existence have been spent in a state of stupor, or even coma. It is certain, however, that, in many cases in which no symptom of this kind could be recognised, the very same appearances were found on dissection. This diversity in the results of cases, apparently the same, has induced many pathologists to refer those examples of effusion which were indicated by no symptom during life, to the process of transudation after death. That this was an actual fact, was very probable for several reasons; but no direct proof was adduced to support the assertion. I consider myself somewhat fortunate in being able to furnish some very interesting observations on the point at issue, from very high and respectable authority. Joseph and Charles Wenzel of Vienna think that many examples of aqueous effusion in the ventricles do certainly take place after death; and, to confirm their opinion, detail the following cases.*

"On the 29th of May, 1801, four state criminals suffered at Mentz the punishment of decapitation by the guillotine. Thirty-five minutes after the punishment of the last, the body of the delinquent who was first beheaded, was brought, along with the four heads, to the theatre of Ackermann, Professor of Anatomy. The craniums of three of these were immediately opened, and

* Josephus et Carolus Wenzel De Penitiori Structura Cerebri Hominis et Brutorum. Tubingæ apud Cottam. 1812. Fol. p. 199, *et seq.*

the brains submitted to careful examination, in the presence, it would appear, of the two Wenzels.

"On examining the brain of the individual who suffered last, a little pure fluid (lympha) was found in the inferior and in the posterior corner of the left lateral ventricle. In the lateral ventricle of the right side, and in the fourth and fifth ventricles, not the slightest trace of fluid appeared. The cavity of the middle septum could not be detected. In neither of the cavities of the second brain could a drop of fluid be seen.

"The third brain presented the same phenomena as the one which had been first submitted to examination; but lymphy or watery fluid (lympha) was observed at several spots between the arachnoid membrane and the pia mater. A small quantity of the same kind of fluid was found in the pericardium of the individual who was decapitated first.

"The brain of the fourth criminal was allowed to remain untouched till the morning of the following day (May 30th), when it also was subjected to examination, at an interval of about fifteen hours after death. The result was, that in the ventricle of the middle septum, in each division of the lateral ventricle on the left side, and in the fifth ventricle, the lymph was in general more copious than in the first and third brains."

From these observations, the Wenzels conclude, that, in brains which are examined immediately after death, while the temperature of the body still continues, the presence and quantity of the fluid of the ventricles is in the same proportion as in brains which have been examined at a much later period. This fluid is in truth, they remark, sometimes present, and sometimes it is not found; but when it is, its presence seems to depend on no other cause than that which occasions the pericardial fluid, and what is found between the membranes of the brain; for the nature, condition, and end of both, seem to be the same, and purely in consequence of the entire interruption of the circulation, and the extinction of the power of absorption after death, does that assume the appearance of an aqueous fluid, which, during life, was such a delicate vapour as to escape the eyesight.

These remarks, I conceive, go far to explain many of the cases of watery effusion into the ventricles; but they cannot, I am satisfied, account for the whole of these; for, if all the cases of the description we are considering depended on such a cause, we might expect to see fluid in every brain which we examined; and we should certainly observe a relation between its quantity and the time which has elapsed since the extinction of the vital actions. So far, however, as my observation goes, I am not aware that this has been established.

I believe, therefore, we must admit, that effusion actually

does take place between the membranes, and on the central surface of the brain, without in every instance producing very conspicuous effects on other organs and functions. It is, in short, a condition of the cerebral membranes which may occur either with or without affection of the organs of sensation and voluntary motion; but I have in general remarked, that, in those subjects in whom the comatose affection during life had not manifested itself, the effusion had taken place in a very slow and gradual manner. It is, in truth, in consequence of this slow and gradual change, that the nervous system of the sensitive and locomotive apparatus is so little affected; it becomes, as it were, insensibly accustomed to the compression thus occasioned; and it is only when the effusion is extreme, that any striking effect is experienced. Of this we have a remarkable instance in the phenomena of the chronic hydrencephalus, in which the same effusion takes place which we have now been considering. The serum is poured forth in great quantity between the arachnoid membrane and the pia mater, and in the central or interior surface of the brain, where the choroid plexus is likewise affected. Yet, although this state continues in some instances for many years, we rarely meet with any affection of the organs of sensation or locomotion. The last example of this disease, in which I had occasion to examine the cavity of the head, afforded a complete proof and illustration of the accuracy of these views. The subject was a child of about three years old, who had laboured under the symptoms of chronic hydrencephalus for about eighteen months or two years. The habit of this patient was strumous; he had the rachitic cranium; and the osseous texture was exceedingly soft, spongy, much lighter, and less compact than natural. Its size was considerably augmented, and the brain was proportionally distended. A very great quantity of fluid was found between the pia mater and the arachnoid membrane, the cells of the choroid were unfolded and distended, and the ventricles were considerably extruded by the fluid which they contained. Although it might have been presumed that this fluid would have speedily produced a fatal compression of the nervous system, yet the child existed in comparative health for many months, and died apparently in consequence of a disorganized state of the whole of the right lung, and a considerable portion of the left. Every thing, in fact, that we yet know regarding this condition of the cerebral membranes, tends to establish this great general maxim, —that an effusion, nay, to speak more generally, any morbid change, which takes place in a short time, and occasions a sudden compression of the brain, produces death very rapidly; whereas any change which comes on slowly, and produces its effects in a gradual manner, and after a long period, seldom

proves *suddenly* fatal. Under the former head, we arrange the effusion which takes place in the course of contagious fever; that which is the result of acute hydrencephalus; that produced by the serous apoplexy, as I shall afterwards show; and perhaps an effusion which takes place in consequence of suppression of urine, and traumatic gangrene. Of the latter we have an example in the effusion resulting from chronic hydrencephalus; in an effusion attending the development of morbid growths or tumours; and perhaps an effusion which frequently succeeds to the last stage of diseases of the heart.

*As an effect of this effusion of fluid on the central surface of the brain, I must notice here a very interesting and curious change which sometimes takes place in the lucid septum. This consists in the progressive attenuation, perforation, and rupture of the septum, so as to form in it a considerable hole, by which the two lateral divisions of the central surface, called the ventricles, communicate directly and freely. At first the septum, thin as it is, becomes thinner, and actually translucent; very soon, a number of minute perforations appear in it, of no determinate figure, but which, gradually enlarging, are separated from each other by thin capillary threads of the cerebral substance, of which the septum is composed. In this state, it looks like a small piece of network placed between the mesolobe and fornix; and a very trifling force applied to it will destroy the interlacement of cerebral threads, and produce a hole of an elliptical figure, the larger diameter of which will be antero-posterior. I have observed this progress in the results of many cases; but the reticulated appearance was remarkably distinct in the septum of a man who had died with symptoms of contagious fever, and in whom this effect had succeeded to the effusion of pellucid fluid. This had taken place both beneath the arachnoid membrane and in the ventricles; in consequence of the latter of which, the mesolobe was elevated from the fornix; and the cerebral matter of the septum was so stretched as to form the network appearance which I have noticed. In this instance, the gentlest application of the finger of one of the spectators produced the breach of the reticula, and the retraction of the fibres, so as to form the elliptical aperture, and complete the communication. I have been since informed by Mr. Staff-Surgeon Schetky, that he has lately seen an equally well-marked instance of the reticular septum, in consequence of watery effusion, in the brain of a man who had died of very general organic disease.

* This paragraph, down to "general organic disease," is the only one not in the Records of the Society. I however consider myself entitled to transcribe it here, as the substance was actually written four years ago, and part of it published in the Clinical Reports of Dr. Duncan, junior.

It has been said that the arachnoid membrane sometimes secretes purulent matter. If it does so, it must be, I presume, in the course of those actions which take place in consequence of wounds of the head or fractures of the cranium; for I am disposed to doubt its spontaneous origin. I have seen, between the arachnoid membrane and the pia mater which covers the fossa of the oculo-muscular nerves, the annular protuberance and pyramidal eminences, a quantity of opaque substance, of white yellow colour, which, at first sight, resembled purulent matter. When, however, it was more carefully examined, it turned out to be a quantity of coagulable lymph, which had been effused in this situation. As it resembled purulent matter so closely, that, on a cursory glance, it might readily be mistaken for it, I think it may not be unwise to doubt the assertion regarding the secretion of this fluid, till we have a greater number of well-marked and unequivocal cases.

The last morbid appearance which I shall notice, as presented by the arachnoid membrane, is another instance of an alteration taking place in it, but depending on a diseased action, which originates in another part of the encephalic organ. Instead of the smooth and glistening aspect which characterises the sound condition of this envelope, the pathographical observer remarks, that it has become unusually dry, and seems to be no longer lubricated with the fluid which exudes from its non-adherent surface. I do not at present remember that this change in the arachnoid membrane has been mentioned by authors on the subject; but I am quite satisfied of this fact, that it has never been remarked to be connected with a particular condition of the central surface of the brain. I have invariably found it in the true acute hydrencephalus of children, and, so far as my observation bears me out, it indicates the presence of water in the cerebral cavities. I do not at present remember a well-marked case of this disease, the acute hydrencephalus, in which I did not at first recognise the dry, dull, and lustreless appearance of the arachnoid membrane, and find, at the same time, a quantity, more or less considerable, of limpid fluid in the ventricles. My limits will not permit me to offer any observations on the link which seems thus to connect the actions of two different parts of the same organ; but, according to the usual principles of medical reasoning, I believe it will generally be considered as an example of the transference or modification of a natural action, in consequence of the excessive degree of a diseased one. The symptoms attending this appearance are abundantly well known, and require, on that account, no notice from me; but it might, perhaps, lead my readers to misunderstand what I have advanced concerning this state of

the arachnoid membrane, if I failed to notice certain discriminating circumstances with which it is accompanied.

First, then; it might be understood that, whenever the cerebral cavities are expected, from the symptoms during life, to contain fluid at death, then the arachnoid membrane must be dry, dull, and destitute of its ordinary glistening aspect. This, however, it might be remarked, is contradicted, both by what is generally known on the subject, and likewise by what I have myself said in a previous part of this paper. Fluid may be found in the cavities formed by the central surface, and the arachnoid be unchanged in its appearance; or it may exhibit the appearance which I have described, when speaking of the aqueous state of the pia mater. There is, therefore, a certain order of affections in which the arachnoid membrane retains its shining aspect, and there are others in which this is no longer observed. The latter is what takes place in the acute hydrencephalus, and is, in truth, the only pathographical phenomenon which particularly distinguishes this disease from the other morbid conditions in which fluid is found in the ventricles.

Secondly; a great number of affections of the head have a common termination in effusion on the two surfaces of the brain, the convoluted or exterior, and the central, or that of the cavities. We observe these in persons of all ages and habits—in the young and in the old, in the robust in certain circumstances, and in the feeble and strumous habits; and we recognise them as a sequel to many different or even opposite maladies;—to contagious fever, to dropsy, to diseases of the organs of respiration and circulation, to traumatic fevers, &c. We observe this effusion taking place in periods of very short duration, and sometimes only in the course of many weeks or months. Lastly, we see it, in some instances, attended with very characteristic and formidable symptoms, and at other times producing so little effect on the other functions, that the most sagacious observation cannot, in certain circumstances, be led to suspect it. Are these effusions the same with that of the acute hydrencephalus? If they are not, in what respect do they differ from it? I cannot indulge in the hope that I am to explain this difficulty entirely; but I trust that the following remarks will tend to elucidate it in some degree.

From the historical details which I have already given of the circumstances which attend the morbid appearances I have mentioned, it would appear that, in the instances of effusion between the arachnoid membrane and the pia mater, the diseased action is restricted, with great accuracy, to the vessels of the latter membrane, whether enveloping the convolutions, or penetrating into the central surface. This I conceive to be almost

undeniably established by the observations which I have already made; and I think we may with equal certainty say, that the pia mater is never, or at least very rarely, affected in the acute hydrencephalus. This disease, on the contrary, wherever we admit that it is peculiarly seated, has a decided influence on the arachnoid membrane. If we could believe with Bichat, that it penetrates into the central surface, the argument would be still more conclusive. But even this is not necessary to prove that the arachnoid is exclusively influenced in the action of acute hydrencephalus, and is not affected in the other maladies which produce effusion. There is nothing absurd in supposing, that if the action in which hydrencephalus essentially consists, be restricted to the central surface of the brain, or to the epithelion, if you will, which invests this surface, the functions of the arachnoid may be simultaneously affected, so as to induce the peculiar aspect by which it is characterized in this disease.

There is perhaps no occurrence which is so unaccountable as the appearance of air in the vessels of the pia mater. There is not the slightest doubt that it is very frequently observed; and it is possible that those who explain it, by referring it to the process of decomposition after death, are correct in the majority of instances. But there are still examples of this phenomenon occurring in circumstances where we have no reason to suspect that the elementary particles, either of the blood or of the vascular tunics, have reacted on each other, and entered into new combinations.

We are not told whether the gaseous fluid be found in arterial, or in venous tubes exclusively; we may readily conclude, that if it be found in the one, it will speedily seek a path for itself into the other; and all that remains for us to do, is to inquire by what means it gets into the vascular system; for we can easily see, that if it once get there, it will shortly be found in the cerebral vessels; and we can scarcely believe it to be generated in them. Bichat seems to consider it as quite certain, that the air passes immediately from the bronchial extremities into the blood-vessels, without previously entering the cellular texture. This opinion he rests on the fact, that among several subjects in which the arteries and veins of the head and neck contained blood mingled with numerous air-bells, one he had ascertained had expired suddenly in a convulsive motion of the muscles of the chest.* I very much fear, that it will be impossible to derive any thing satisfactory from such explanations, which certainly do not get over all the difficulties. It is perhaps of little consequence how the air enters; it is quite certain

* Recherches Physiologiques sur la Vie et la Mort, par Xav. Bichat. An. XIII. (1805.) Part II. Article 9vieme § 1. and Notes, pp. 284–287.

that it is found there, and that it extinguishes life very suddenly. On the mode in which it acts, different opinions have been given. Morgagni, who has recorded many striking examples of the event, had recourse to the obvious and simple doctrine of a fatal compression. This idea, however, was much too gross and mechanical and palpable for Bichat, who, though he agreed with Morgagni in thinking that it slew the patient by operating on the brain, yet denied that the small quantity which was found could really compress the organ, and contended, that it produced an interruption or annihilation of the cerebral action.* Although the acute defender of this refined and somewhat mysterious speculation, has supported it with numerous proofs and arguments, yet I must either admit my own incapacity to perceive their force, or I must say, that they are neither satisfactory nor conclusive. More recently, an opinion, which was pretty generally admitted previous to the time of Bichat, has been revived and defended with great zeal and ability by a physiologist who aspires at the high office of correcting what is erroneous, and completing what is imperfect, in the doctrines of the distinguished author of the Treatise on Life and Death. M. Nysten, to whom I allude, has performed many experiments on the mode in which death takes place when air is found in the vascular system; and these have led him to conclude, that the brain is never the organ affected by it, but that, in certain circumstances, the heart suffers primarily, and in others the lungs. When air, he says, is found in the venous system in very great quantity, it first distends mechanically the right cavities of the heart, and then destroys its contractile power; so that the individual is killed, first by the interruption of the pulmonary, and then of the general circulation.† When, on the other hand, air is found in the venous system, not in sufficient quantity to effect the death of the heart, it forces itself gradually into the branches and divisions of the pulmonary artery, where it produces a singular degree of obstruction, and impedes the complete filling of the bronchial extremities. A peculiar action resembling *peripneumonia notha*, or *bronchitis* takes place, and the individual dies of suffocation in the course of the fourth day.‡

Dr. Powell has mentioned examples of tubercles forming on the pia mater, especially where it sends processes into the cerebral anfractuosities.§ His description is not sufficiently minute for

* Ibid. Part II. Article 2de § 2. pp. 167–169.

† Recherches de Physiologie et de Chimie Pathologiques pour faire suite à celles de Bichat sur la Vie et la Mort. Par P. H. Nysten. A Paris, 1811. Premiere Section. Article I. § 1. pp. 27, 28, and 34.

‡ Recherches, &c. Premiere Section. Article I. § 2. pp. 37, 38, 39.

§ Transactions of the Royal College of Physicians, London. Vol. v. p. 233.

the pathologist; and my own opportunities do not enable me to add any thing important, either to our knowledge of the morbid appearances, or to the semeiographical relations of this species of disorganizing process.

The cerebral surface consists of two great divisions, which are continuous, and communicate with each other in a very direct manner. The first of these which is applied throughout its whole extent to the dura mater, or its productions, is exterior, is marked with the bodies denominated convolutions, and is what we name the *convoluted* surface of the brain. The second is interior, is marked with objects of a different description, forms those cavities more generally known in the common anatomical works under the name of ventricles, and constitutes what we name the *central* surface of the brain. Each of these surfaces may be the seat of effusions of serum, of blood, of puriform fluid; or they may be broken down, or disorganized by these effusions.

The effusion of serum on the convoluted surface, is a very rare occurrence; that on the central, which is infinitely more common, has already occupied our attention.

Blood may be extravasated on the convoluted surface of the brain, in several different circumstances. That which is the effect of external violence, properly comes under the class of injuries of the head, and does not therefore form a necessary part of my duty to consider. When it takes place spontaneously, it produces that assemblage of effects on the functions of sensation and locomotion, to which physicians have given the name of apoplexy. It may occur in almost any point of this surface; but I think it is most frequently observed somewhere along the inferior region. The anfractuosities contiguous to the fissure of Sylvius, are very generally occupied with bloody extravasations; and this may be attributed to the vicinity of a numerous cluster of vessels. The *white perforated spot* of Vicq D'Azyr,* which is remarkable for admitting a great number of vessels from the sylvian artery into the substance of the brain, has been observed by Joseph and Charles Wenzel of Vienna, to be more frequently affected than any other region of the convoluted surface.

I have frequently seen the anfractuosities between the cerebellic folia considerably distended with effused blood, and an extraordinary number of blood-drops on the exterior surface of the annular protuberance.

* Substance blanche que j'appelle *perforée*. Cette substance, percée d'un grand nombre de conduits plus ou moins verticaux pour le passage d'un grand nombre d'arterioles, se trouve située vers le tubercule d'ou sort le nerf olfactif, entre là racine externe de ce nerf et le trajet du nerf optique. Vicq D'Azyr, Planche xvii. Figure premier. Parisian edition, 1786.

Similar appearances may be observed on the central surface of the brain; and there is scarcely a single body found in that surface which has not been recorded by some pathologist as the seat of bloody effusion. The parts, however, most frequently affected, are the striated body, and optic thalamus.* Whether these hemorrhagic events occur on the exterior, or the interior surface of the brain—whether they take place on the optic thalamus, or the striated body, or any other part,—they are uniformly attended with one of two effects. If the quantity be considerable—if it be effused suddenly—if active remedies be not used—or if, when used, they be inefficacious; then a mortal compression may speedily kill the patient, and the appearances on dissection will be nearly one or other of those which I have already noticed. If, however, by the proper use of remedies, the effused blood be prevented from acting so long and so completely as to extinguish life immediately, then further changes are found to take place, and these go on until they have so altered the natural structure, that life can no longer be continued. The effused blood breaks down, softens and disorganizes the surface, which, instead of presenting its ordinary polished and rounded appearance, becomes rough, irregular, soft and pulpy.† This change, which may take place in the course of about from seventy hours to eight days, has been noticed by Mr. John Hunter, and some other authors. It has generally been considered as a species of suppuration; and perhaps this action may succeed to some inflammatory state of the cerebral matter, induced by the presence of the blood, which then acts like a foreign body. I am, however, very doubtful if we can suppose the cerebral matter ever to undergo the suppurative process, unless in consequence of external injury; for I believe that spontaneous inflammation taking place in this organ, proves fatal long before it passes into the suppurative stage. I am aware, that, in advancing this opinion, I expose myself to the charge of contradicting the descriptions given by very high authorities; but the observations of Dr. Baillie, the writer to whom I allude, have, I conceive, been derived either from cases which were really and truly extravasations of blood, or the appearances produced by the sequelæ of injuries of the brain.

Effusion of blood into the cerebral substance occurs very fre-

* Josephus et Carolus Wenzel de penitiori structura cerebri Hominis et Brutorum. Tubingæ apud Cottam, 1812. pp. 97. 98, 99. This observation, which I find did not escape Morgagni, has been particularly dwelt upon by M. Lallemand, who finds the *softened* brain most frequently in this site. Letter I. No. 22. III. and IV.; and in the last paragraph he ascribes this frequency of morbid action to the same anatomical fact, viz. the multitude of arterial branches which enter the cerebral substance at the beginning of the sylvian fissure, or that part which we have named the white perforated spot.

† De Penitiori Structura, &c. p. 99, 100. Observat. 3tia et 4ta.

quently in the grey matter of the convolutions. This matter is arranged in fibrous lines, the direction of which is perpendicular to that of the convoluted surface, and the blood is generally effused in the interstices between these lines. The effusion, however, is seldom restricted to the grey matter, but in most cases penetrates into the white matter of the hemispheres. A very common situation for this event is the grey and white substance corresponding to the white perforated spot of Vicq D'Azyr. This part, which is situated at the interior extremity of the fissure of Sylvius, corresponds to the matter which is found to form the striated body, and, when examined more carefully, it is found, that the vessels which enter at this part of the organ continue their course into that peculiar substance to which the name of the Capsule of Reil has been given. The whole striated body, the capsule of Reil, and the grey matter of the sylvian fissure, are on this account, and from the vicinity of these vessels to the trunk of the internal carotid, exceedingly vascular; and it cannot be matter of wonder that hemorrhagic effusion should be most frequently observed in this situation.

Next to the striated body, the annular protuberance is the most frequent seat of that morbid change which we are at present considering. The mode in which the extravasation takes place, depends on the structure of this object. It consists of a twofold series of fibrous lines, the direction of which is perpendicular to each other. One set is arranged on the longitudinal, the other in the transverse, direction of the body. It is in the interstices of the latter order of lines, that the bloody effusion is most frequently observed. A very well marked case of this kind occurred to my observation about two years ago; and in this blood was found so copiously and regularly deposited between the transverse fibres of the protuberance, that an eminent medical friend thought it worth recording by an oil painting. Regarding the effects of these changes of the protuberance on the system at large, Bichat says, in a manner equally brief and expressive, that although numerous cases of lesion of the brain proper and the cerebellum, without affecting the other organs, are on record, yet he knew of none of the protuberance which was not fatal.*

In the cerebellum, I cannot say that I have had opportunities of remarking any peculiarities which may not have occurred to other observers. I think that the capsule of the *corpus dentatum*, or moriform body, is generally much more vascular than natural in apoplectic subjects; and I have seen the moriform

* Traité d'Anatomie Descriptive. A Paris, An. XI. (1802), tome iii. Article 5quieme, p. 108.

prominences nearly obliterated by the quantity of blood effused on them, or into their structure.

The medulla oblongata has been too seldom suspected to be the seat of disease of this nature, and too few facts have been recorded concerning it. I have seen the exterior surface very rarely occupied with blood. The interior substance, I am satisfied, is much more commonly affected. I have, however, had too few opportunities of personal observation, to say any thing very decided; but I have reason to believe, that the capsule of the *corpus dentatum* of the olivary eminences is generally the seat of considerable extravasation in apoplectic subjects. It is only from future observation in many examples, and conducted with patient and persevering attention, that we can expect any precise or accurate information on these dubious points.

The same remarks which I submitted on the effects of these hemorrhagic effusions when taking place on the surfaces, apply to them with equal force when occurring in the substance of the organ. If the effused blood do not immediately cause every vital action to cease, it acts as a foreign body, breaks the fibres, or compresses the globules of which the part may happen to be composed. This mechanical destructive change is succeeded by vital ones, which act in a way exceedingly obscure, but give rise to effects sufficiently obvious. The part contiguous to the effusion becomes soft, pulpy, and irregular. When examined with the microscope, it presents no regular or uniform structure similar either to the surrounding parts, or to that which it presented previous to effusion. A number of minute granular eminences, very different in size and in figure, surrounded with a semifluid substance, is all that the most careful inspection can recognise. I am quite satisfied that this appearance has been often set down by authors as the abscess of the brain; but I cannot admit the assertion to be correct, till they have previously defined what they mean by the term, *suppuration of the brain.** If, indeed, the cerebral matter, in consequence of the presence of a substance which is heterogeneous to it, and destitute of the living properties, be made to assume an action analogous to the inflammation of the other textures; if, in short, it commence a sort of effort to remove the noxious substance, then perhaps we may allow, that the broken down and disorganized parts are actual imposthumes. This view of the effects of the cerebral hemorrhage, however, has, so far as I know, been offered by no one of those pathologists who have spoken so familiarly of the cerebral abscess.

* Recherches Anatomico-Pathologiques par F. Lallemand, Lettre I. et III. especially No. 27. § xii. xiii. xiv. pp. 474–489.

Another effect analogous to that which I have now described succeeds, in some very rare cases, to the extravasation of blood into the substance of the brain. When blood has been effused into the hemispheres without terminating immediately the patient's existence, it may disappear very nearly in the manner I have noticed; but a quantity of serous fluid occupies its place, and fills the cavity, which is never obliterated. This is a very rare appearance; but it is, nevertheless, occasionally met with; and I think that the few cases which have been observed, have been found in the brains of persons who have survived an apoplectic attack for many years, and, at the same time, suffered under a palsy more or less general.

In the substance of the brain, bodies of a greater consistence than the surrounding parts are sometimes met with in the course of dissection. To these the general name of tumours has been applied. The most rational view of their origin and nature would lead us to deem them, in general, to be cerebral matter considerably changed by some morbific cause, and no longer presenting its usual organized appearance; but generally much harder, resembling sometimes cheese, sometimes soft cartilage. The observations and dissections of the two Wenzels lead to the following results—that the brain proper may be the seat of disorganized portion of three different kinds; 1*st*, A tumour of an adipose nature; 2*d*, A substance of an osseous nature; 3*d*, A substance resembling scirrhus. Among the great number of brains which these anatomists have examined, only two presented the adipose degeneration. These were found toward the exterior or convoluted surface, but penetrated pretty deep into the substance of the organ. The aspect of the interior structure is subcinereous, its consistence solid and firm,—approaching in some parts to bone. Of the scirrhous degeneration, these anatomists met with only two examples, both of which were situated on parts connected with the central surface;—an important mark of difference from that of the adipose tumour, so far as we hitherto know. The ossific change is infinitely less common. With regard to the exclusive seat of all these disorganized portions, it is a general fact, that none have been hitherto recognised in the cerebellum.*

There is perhaps nothing in the whole of pathological science so puzzling and contradictory as the semeiography of these bodies. I have had occasion to know or observe several of them which seemed to have originated under a great variety of different circumstances. The writings of pathologists are full of cases in which tumours had been found in the brain after death, when no morbid appearance, at least not of that kind,

* De Penitiori Structura, pp. 104, 105.

nor in that part, had been anticipated;—cases in which, after obstinate and violent cephalaea, substances had been found in the brain, which had first irritated and then compressed it;—and cases in which substances very unlike each other had produced very different effects on the other functions. I have in my collection two small bodies of an irregular form, but not exceeding the size of a small bean, which were found in the anterior lobes of the brain of a young person who had died of chorea,—a disease very rarely fatal. It is well known, that most, of these tumours, as they have been named, give rise first to epileptic, and then to apoplectic symptoms; but it is remarkable, that, in very many cases of epilepsy, no such appearance has been found. I knew very intimately the subject of one of these examples of disorganized brain denominated tumour, in whom the first symptoms appeared in the course of a very severe febrile disease, accompanied with affection of the head,—acute pain, double vision, vertigo, and deafness alternating with *tinnitus aurium*. Yet life was continued in exceedingly good health for more than a year; and death took place in consequence of phthisis, fully eighteen months after the original malady.

Amidst such chaotic discordance of symptoms and morbid appearances, it is difficult, if not impossible, to recognise any thing leading to general principles; and we must either wait until some more fortunate observations shall supply us with the means of discovering greater regularity in the effects of such substances, or we must despair of ever attaining any accurate and precise method of ascertaining the presence of cerebral disorganization, or perceiving the slight and mingling shades of difference in symptoms which closely resemble each other, but indicate diseases pathologically unlike. Before I conclude this part of my subject, I have a few remarks to offer on morbid changes occurring, or said to occur, in particular bodies which are not with propriety referrible to any of the above heads. I allude to changes in the *hippocampus major*, changes in the *conarium* or pineal gland, and changes in the *hypophysis*, or pituitary gland.

The *hippocampus major* is, like other objects of the central surface, liable to morbid alteration; and the Wenzels have attempted to convert these to a use somewhat singular. They record three cases in which this body deviated from its usual size, or in which its exterior plate was preternaturally soft and pulpy, or adhered to the contiguous surface of the *cornu*.* The individuals in whom these changes were found, laboured under melancholy, insanity, or fatuity; and the anatomists whom I have mentioned think, that a rational connexion may be esta-

* De Penitiori Structura, &c. pp. 142–3. 209, 210—214.

blished between the two circumstances. It is hardly necessary for me to say, that conclusions of this nature can never be relied on; and unless we have a greater number of observations and cases, we cannot, with any propriety, admit the truth of them. I am so much the less disposed to trust to the opinions of these very indefatigable and meritorious anatomists on this point, that repeated and careful examinations of the object in question, have induced me to form opinions entirely different from those which they have given regarding its interior structure.

The body named the *Conarium* or Pineal Gland, consists of two parts, the *Conarium proper*, and the *Acervulus*. The figure, appearance, and colour of this body, are well known In examining brains with a view to elucidate the nature of diseases, medical men had often met with small sabulous or calcareous particles; and it was long supposed that this was a change in the structure of the part, which could not fail materially to affect the functions of the entire organ. Sœmmering was the first who called this doctrine in question; and proceeded to show, by numerous cases, that scarcely any person arrived at the age of puberty, although enjoying the best health, could assure himself that his conarium did not contain calcareous matter.* The period after which Sœmmering always found this deposition, to which he gave the name of *Acervulus*, was the 15th year; but this has been recently doubted by the Wenzels, who declare that they have seen real stony particles so early as the 7th year; and a substance very similar a few months after birth.† I am myself satisfied that I have seen, in the pineal glands of young children whose ages I could not ascertain, but who certainly did not exceed nine or ten, small sabulous particles; but my personal observation does not enable me to say how far the Austrian anatomists are correct as to earlier periods.

The *acervulus* may occur in three different situations; it may be found either in the medullary band which connects the conarium to the optic thalami, or in the pit which unites this part with the body of the conarium; or lastly, in the conarium itself. In infants, before the 7th year, it is never found in the substance of the conarium, but generally in the plate which connects it to the thalami. The most ordinary place for it in adults, is the pit between the conarium and its peduncles; and in aged subjects, it may be found in all the three situations.

It is now perfectly certain, that whether this deposition be found in the one or in the other of these situations, not the most trivial effects on the cerebral functions take place. We cannot, indeed, account for this, or pretend to answer the question of

* S. T. Sœmmering de Acervulo Cerebri in Ludwig Scriptorum Neurolog, minorum Delectu. Lipsiæ, 1793. Tom. iii. pp. 322, 323, &c.

† De Penitiori Structura, pp. 155–157.

Sœmmering; but it is of some consequence to know what is actually morbid and what is healthy, and I must rest my apology for the length of these observations on this circumstance.

Joseph and Charles Wenzel have laboured with much assiduity to show, that there is an intimate connexion between the conarium and the hypophysis or pituitary gland, and between both of these bodies and certain conditions of the entire system. But the examples which they adduce, are a great deal too few, and their observations are a great deal too vague to allow these conclusions to be yet ranked among the class of ascertained pathographical facts.

Having given this view of the first part of my subject, I have now to make a few very brief remarks on the second general division; the connexion subsisting between morbid states of the brain, and morbid states of other parts of the body. It might be possible to extend these to a considerable length, but I have every reason to be somewhat select on this subject; and I shall restrict myself either very nearly or wholly to what I have derived from personal examination and observation.

The account which I have already submitted, of the morbid changes to which the different parts of the brain are subject, will readily suggest the very great influence of the blood-vessels in accomplishing these changes. It is, in short, exceedingly interesting to observe, that the sanguiferous system is a very powerful agent in effecting many events which take place in the nervous system. When I make these remarks, I trust that I shall not be misunderstood, nor be thought to favour the notion of any vague or mysterious connexion subsisting between these two component parts of the animal body. It is the practice of many to explain all changes in the animal body by the operation of a single set of organs. Some, for example, erect their altars to the vital principle; a great many to the sanguiferous system; and not a few consider the brain and nerves as the universal agents of good and evil. But he who attempts to explain all things by these isolated means, only demonstrates the impossibility of explaining any; and the accommodating and convenient application of which each admits to every possible case, shows satisfactorily that they cannot all be in the right. It is too much for any individual to expect that he will be entirely exempt from the general sources of fallacy and error to which all seem to be liable; and a gradual approximation to the true and natural representation of causes and effects, and of the connexion of events, whether simultaneous or successive, is all that the most sagacious observation and most penetrating intellect may hope to attain. To some these remarks may seem very unequal to the real importance of the

subject; but I have some reason to believe, that the observations with which this Essay is to conclude may be new to some, and therefore unexpected; unnoticed by authority, and therefore unwarranted; and I have thought it not unnecessary to anticipate any misconception to which they might give rise. I shall not attempt to show, that most of the affections to which the brain is liable depend on a peculiar condition of the heart and great vessels; but my present purpose is, to make some remarks on the connexion between morbid states of the heart and great vessels, and morbid states of the brain and its membranes.

It is very well known, that, when the heart is diseased, it never fails to derange the functions of many other organs, and to induce effects which had been long mistaken for primary diseases, till more accurate and intimate knowledge of the maladies of the circulation, showed that they were mere secondary results. Many disorders to which the organs of respiration are liable, were habitually ascribed by practitioners to affections of the lungs and their appendages, till it was found that a very great proportion indeed of these was actually induced by morbid changes which had taken place in the heart, and the parts immediately connected with it. It does not appear, however, that any pathologist has particularly examined those effects to which the diseases of circulation give rise in the cerebral organ; yet it cannot be doubted, that a very real and great influence is exercised by the former on the latter. Mr. Corvisart, who has observed with such accuracy, and described with such clearness the diseases of the heart, scarcely hints at the subject, and dismisses it in a single brief and rapid section; and yet he had before his eyes a very striking example in the person of a distinguished member of the profession.* I cannot account for the singular oversight, as I deem it, in any other way, than by reminding my readers how very natural it is to overlook, in pathological autopsy, any changes which may have taken place in different organs, when one presents enough, as it has been said, to account for the symptoms. A man, for example, falls down in a fit of apoplexy; he is carried home, and treated in the usual manner; he perhaps recovers, to fall a speedy victim again to his disease; perhaps he dies, and his head is inspected. The physician finds blood or serum extravasated, and concludes his examination, perfectly satisfied with the result. He naturally does not think of carrying his autopsy into other organs, when the symptoms are accounted for according to the usual doctrines; and he never thinks of tracing the chain of morbid events any further.

About five years ago, my attention was forcibly called to this

* Malpighi.

subject by a case, the particulars of which I shall detail very briefly. I then thought it might be an exception to the general character of cerebral diseases, and I consequently endeavoured, by all means, to ascertain whether it were really so or not. I have since had occasion to examine a very considerable number of cases, and have found that the connexion between the two events, of disease of the heart and disease of the brain, is a great deal more common than I anticipated; and I have known cases which were examined by professional friends, in which the same connexion existed, though it did not occasion particular remark. It would very much exceed the limits within which I must restrict this paper, were I to detail all these cases; and it would certainly be mistaking, in a great degree, the object which the author of a paper of this nature ought to have in view. I shall therefore notice only three examples of the fact; and, as these were very characteristic, they will illustrate and confirm the opinion I have advanced quite as forcibly as if I had described as many hundreds.

1. P. E., aged 22, was attacked, rather in a gradual and insensible manner, with constrained and difficult breathing, and a sense of fulness, tightness, and anxiety in the chest. These symptoms did not attract particular attention, until he observed his feet becoming œdematous, his urine scanty and high-coloured, and, above all, his sleep interrupted with frightful dreams, attended with violent palpitation. These complaints at length compelled him to take medical advice; and the physician whom he consulted, justly ascribing the symptoms to hydrothoracic effusion, began, however, very soon to perceive, from the irregularity of the pulse, the violent and unequal action of the heart, and the progressive course of the symptoms, which presented neither alleviation nor interruption, nor seemed to be connected with any state of the gastric viscera, that some disorganizing process had commenced, and was going on in the central organ of circulation. Notwithstanding the diligent employment of such means as were calculated to remove the effused fluid, and control the action of the heart, the disease advanced progressively; and one day, after feeling an extraordinary degree of giddiness, with some tinnitus aurium, he staggered towards his bed, and fell down in a state of hemiplegic paralysis. Leeches and a blister were applied to the head, purgatives were administered, and the feet were irritated with sinapisms, but his recovery was merely partial; the slowness and irregularity of the pulse, which was generally about 42, and almost never exceeded 54, continued; the sense of suffocation was not remarkably urgent; but this patient died, with symptoms denoting at once syncope and paralysis, exactly three weeks after the attack of hemiplegia.

A notable quantity of fluid was found between the arachnoid membrane and the pia mater, especially in the vicinity of the venous trunks. About three tea-spoonfuls were found in the ventricles. Both the convoluted and the central surface were, in other respects, sound; but in the region of the striated body of the right hemis-

phere, a recently formed cavity was found in the cerebral substance. This disorganized space was about one and a half or two inches long, and one inch broad; its surface was rough, irregular, and hard; its colour was rather redder than natural. In the heart, the left auriculo-ventricular opening was very much contracted, and, instead of the mitral valve, with which this opening is surrounded, there could be found only a uniform ring of bone, very irregular in appearance, and various in consistence, but which had been evidently produced by the thickening, enlargement, and disorganization of the valve.

2. A. B., aged about sixty, was found by the police, in the vicinity of the Leith Docks, in a state of insensibility and loss of voluntary motion, which were supposed to arise from external injury or intoxication. When, however, his head was shaved, it exhibited no marks of violence; and though his general aspect was that of a person addicted to habitual ebriety, yet his breath did not exhale the strong and well-marked odour occasioned by recent drinking. He was accordingly blooded very largely; cold was applied to the head; and the bowels were evacuated by means of enemata. In the course of a few hours he became more sensible, moved his limbs, and uttered, in his attempts to speak, inarticulate mutterings. The venesection was repeated; and the other treatment, calculated to obviate the effects of cerebral congestion, was continued. It was evident, however, that he was paralytic in most of the voluntary muscles. Speech was entirely lost; the extremities were moved only imperfectly; the urine was educted by the catheter; and, when he had continued in this condition for about thirty hours after he was first seen, he passed from complete and uninterrupted coma to the state of general death.

The whole convoluted surface of the right hemisphere was occupied by bloody effusion; the cerebral substance corresponding to it was broken down and disorganized; the folia of the cerebellum were covered, and their interstices filled with blood; and the moriform body of the right side was considerably affected. The annular protuberance was more completely the seat of extravasation than any other part. Its inferior surface was quite covered; and when its substance was exposed by incision, blood was found regularly deposited between the transverse fibres of which it is composed. I found the whole aorta, from its origin at the left ventricle, to about two inches below the arch, completely ossified; the semilunar valves thickened and ossified; and the aortic opening much contracted.

3. I lately had occasion to examine the body of a patient who died under circumstances which rendered it impracticable to ascertain any thing concerning her symptoms during life. A woman, aged about 50, had been found in a state of insensibility, which was supposed to arise from inanition. She died so soon after being seen by the medical attendant, that the remedies ordered had not time to operate.

The arachnoid membrane was elevated by a watery fluid deposited between it and the pia mater; the veins of the latter were exceedingly distended with blood; and the whole superior convoluted surface of the hemispheres was occupied with the same fluid. The ventricles contained about 1½ oz. of watery fluid; the choroid plexus pre-

sented several globular hydatiform bodies; and the capacity of all the cavities was much enlarged. Several particles of osseous matter were found along the arch of the aorta; the semilunar valves were thicker and firmer than natural; the left auriculo-ventricular opening was very much narrowed; and the mitral valve was ossified very nearly half way round. The stomach and intestines were filled with fluid, but very little solid aliment was found.

To these I might add a fourth very well marked case, related by Dr. Heberden in the 5th volume of the Transactions of the London College of Physicians. It is there considered as a case of water in the head, unattended by the usual symptoms; that is, that no very obvious phenomena during life indicated any effusion into the cerebral cavities; yet this, with the hydatiform state of the plexus, was found in considerable quantity after death. But the characteristic circumstance to which I wish to direct the attention of my readers is, that the heart was the seat of considerable organic disease. "The valves of the left auriculo-ventricular opening were partially ossified; those of the aorta completely so; and small depositions of bony matter were found on the tendinous portions of the columnæ carneæ. The coronary artery was ossified through its whole extent; the aorta, soon after emerging from the ventricle, formed a true aneurismal pouch." No one, I think, who studies the case of Dr. Heberden, in reference to those which I have related, will venture to assert that it was a true case of hydrocephalus, or that the effusion was not the effect of the disordered state of the circulation. It would be exceedingly easy to multiply examples of this coincidence of morbid structure and action; and I might adduce the instance of Malpighi, who was himself the subject of this twofold disease; but instead of fatiguing the attention by tedious details, I shall conclude with the following remarks.

1*st*, It is quite obvious, that several maladies of the heart, such as ossification of the left side, or of the artery connected with it; ossification of the mitral valve; of the semilunar valves; arctation of the apertures, either auriculo-ventricular, or aortic, have a tendency to terminate in extravasation within the cranium, producing apoplexy, paralysis, or a comatose state terminating in death.

2*d*, It is equally obvious, that although examples of disease of the head, affection of the brain and its membranes, occur spontaneously, yet not a few are the result of a disturbed or irregular state of the circulation, induced by disease of the heart or its appendages. This connexion has not been examined so often as it merited; and there is strong reason to believe, that, if made the subject of particular investigation, it would be proved to be much more constant and uniform than has been anticipated.

3*d*, It is by no means difficult to see how these effects in the cerebral organ result from an irregular and disordered action of the heart. The difficulty which the blood experiences in passing either, 1*st*, through the auriculo-ventricular opening; 2*d*, the aortic orifice; 3*d*, along the aorta, necessarily produces a stagnation and congestion; 1*st*, in the pulmonary veins; 2*d*, in the pulmonary artery; 3*d*, in the right side of the heart. The effect of this is to retard or impede very remarkably the return of the blood from the cerebral veins, and consecutively either to distend them, or, unless they are exceedingly strong and resisting, to rupture them, or to occasion an effusion of the serous part of the blood, as we find in other examples of obstructed venous circulation.

Some authors have noticed the connexion between morbid conditions of the brain, and morbid conditions of the liver. My personal opportunities do not enable me to add any thing to what is already on record; and it is unnecessary to transcribe here what may be found elsewhere.

Edinburgh, *December*, 1820.

Recherches sur la Situation de l'Orifice interne de la Fistula a l'Anus, et sur les parties dans l'epaisseur desquelles ces Ulceres ont leur siege. Par le Docteur F. Ribes.

(From the Quarterly Journal of Foreign Medicine and Surgery.)

It is now twenty years since I first announced in my Lectures on Surgery, that I was engaged in making a series of researches, for the purpose of rendering myself certain, as to the precise situation of the internal orifice of the Fistula in Ano. What I then said respecting the situation of these openings, and on the course pursued by the fistula, has been so imperfectly repeated in some theses, and so much abridged in others, as to preclude the possibility of forming any fixed opinion on these points; and has induced me now to publish the result of my researches on the subject. The following are the reasons which led me to bestow my labour on the points in question:—

At the period when Desault inspired his numerous auditors with his own enthusiastic love of surgery, he also strove to introduce changes into many surgical operations, by means of which he proposed to carry them to the highest degree of perfection; yet he did not induce M. Sabatier, a more learned and discerning man, and whose mind was calmer, and of a far less ardent cast than that of Desault, to adopt all the changes he proposed. Sabatier still continued, and always with success, to treat fractures of the neck of the femur, the clavicle and the knee-pan, by posture and bandages of the simplest cast; and he

likewise continued to make use only of a canulated director and a bistoury in his operations for the fistula in ano. Of the success of his practice in the treatment of fractures, I had had abundant experience; but not being equally convinced of the advantages of his method of operating on fistula in ano, I took an opportunity, when talking with him on the subject, to remark, that it was true, that as often as he found the internal orifice of the fistula near the margin of the anus, his operation never failed of success; but I could not see how he could operate precisely with these two instruments only, on any fistula where he could not find its internal orifice, when that orifice was beyond the length of the finger, or when the intestine was denuded very high up, or when the fistula was of great extent.

In answer, he told me that, during forty years which he had dedicated to practical surgery, he had operated on a large number of fistulæ in ano, both in the Hospital of Invalids, and in Paris; that he had never found any difficulty in the operation; that he had always found the internal orifice to lie near the margin of the anus; that the canulated director had constantly answered his purpose; and that, finally, this operation appeared to him to be so excellent and simple, that it would be difficult to invent a better.

"If," added he, "the internal orifice of the fistula were placed beyond the reach of the finger, a circumstance undoubtedly of extremely rare occurrence, for I have never yet met with one at this distance, it would be necessary to treat it with deference, and only employ gentle means, because we could not in such case perform an operation, but at the risk of subjecting the patient to deadly hæmorrhages.

"Sometimes, it is true," continued he, "that we cannot find the internal orifice of the fistula, nor can we get the director into the rectum by this opening, and it has been advised in this case, especially if the intestines be denuded, to pierce it at the highest possible distance, and to make an incision the whole length of the denudation; and also, that though the sinus remains on one of the sides of the incision, yet it is pretended that a cure will nevertheless take place.

"I am of opinion," said he, "that any person acting thus cannot obtain the desired success; because the disease must necessarily be subject to a relapse, seeing that the source of the moisture proceeding from the excrement, and gradually draining itself into the cells of the cellular membrane, is not obliterated. In such case, when the canula will not pass into the rectum by the internal orifice of the fistula, my advice is, that the operation be delayed awhile. It will probably not be long before we arrive directly into the intestine, and then perform our operation with certainty of success.

"If the rectum be found denuded to a great extent, by the dissolution of the cellular membrane, in case the fistula terminates externally, we judge that union of the sides or bounds of the disordered part cannot take place, because every surrounding circumstance seems to oppose the approach of the parts. It has been thought that, in order to put an end to the disease in such case, it would be best to divide the intestine longitudinally, from the extreme upper to the lower part of the denudation, in order to render the fistulous opening continuous with the margin of the anus, and thus to add the whole depth of the sinus to the natural size of the anus itself. By this means, the whole of the new wound, and the fistulous sinus ought to become firm, to offer a surface, to a certain extent analogous, to that of the bowel itself, and by and bye able to endure the passage of the excrement without inconvenience—or, in other words, it ought to cicatrize.

"I am of opinion, that this procedure cannot be void of danger; because, in dividing the intestine to a certain height, we expose ourselves to the chance of dividing the large arteries, and thus we may give origin to distressing hæmorrhages, not only difficult, but impossible to be cured. As for myself, I even should not operate at all, merely contenting myself with proper applications, and leave the disease to nature, relying on the possibility that the parts may unite. In effect, the rectum always distended in a greater or less degree by the accumulated excrement, fills a large portion of the cavity of the pelvis. The urine also, by its accumulation, distends the bladder, and brings it near the boundary of the lower pelvis. The lower portion of the peritonæum is pushed downwards by the viscera of the abdomen, and tends to fill up the void formed by the sinus. The external sphincter, the ischio-coccygian muscle, and especially the elevator of the anus, still farther fill up the void alluded to; and besides, it is impossible that the whole of the cellular structure of this region should be so destroyed, but that a greater or less quantity must remain, and sufficient, by the subsequent growth of granulations, to unite and bind all parts together. Hence, under all these circumstances, the fistulous opening ought to cure itself without any operation, unless some local or constitutional disorder stand in the way.* If the patient be

* The young practitioner may be liable to be seduced into a deceptive confidence by this detail of the modes by which a natural cure is to be begun and completed, in such a disease as fistula in ano. M. Sabatier's healing agents have been ingeniously called up, but this appearance of the whole, except the last, will avail him nothing. If any spontaneous cure is to be expected, it certainly must be owing to the granulations of the disordered surfaces gradually approaching and interjoining one with another. Pressure may do something in bringing the diseased surfaces nearer, and thus shortening the distances to be closed by the new granulations, but nothing more; and besides, could it be

affected with any wasting disease, and such as naturally tends to produce death, of course any operation would be perfectly useless."

"After what I have stated," said he, on closing his remarks, "and from what you have seen of my practice since you have been near me, and also from what you are well acquainted with from your own experience, you will be convinced that the operation for the fistula in ano is one of the simplest of any in surgery. Yet it is astonishing how a thing so simple in itself, could have been rendered so highly complicated, the whole has been carried to such a degree, that it would almost seem as if difficulties have been created, for no other purpose than that of finding out means of overcoming them."

I attached too great importance to these remarks of my illustrious master, not to form the resolution of examining diligently every dead body that came under my hands, affected with fistula in ano; and the following pages detail the result of my researches. Before, however, I proceed to state what little I have to add to that which is already known respecting the disease, it will be necessary to take a rapid view of the disposition of the anus, and of the parts which enter into its construction.

Of the Anus.—This opening is more or less sunk, or formed into a cavity, according to the nature of the sexes. In the female the hollow is very little, so much so, that it appears almost on a level with the tuberosities of the ischium, which themselves are naturally flattish and little projecting; whereas, in the male, the orifice of the rectum is much more depressed, a disposition undoubtedly arising from the greater projection of the tuberosities, and from their being much less distant from each other, the necessary consequence of the smaller capacity of the bones of the pelvis. In the female we readily discover the union of the skin of the perinæum with that of the internal membrane of

of any great avail, the uncertainty of the bearings of any given pressure from agents constructed as are the bladder and the rectum, &c. which must be constantly changing in size and distribution, would prevent the possibility of its doing much good even under the most favourable circumstances. Could we establish a pressure as constant and as efficacious as that of a suture, we might in the early stages of the disease generally expect a cure by the means of our art: such pressure, we need not repeat, it is impossible to procure, and still more so when it is to be accomplished by viscera, hardly bearing on the diseased part, and ever varying in size, as they fill or are emptied. As healing by the first intention is not to be looked for in ulcerations of this kind, it is on the granulations alone that we must rest for the spontaneous and general adhesion and obliteration of the cavity, if any such can be effected; but herein many difficulties stand in the way of cure, and the chief one is the tardiness with which sinuous ulcers in parts possessing so little of the principles of life, as the cellular membrane surrounding the upper portion of the rectum; and in reality, granulations do shoot with such difficulty as to render a spontaneous cure in any deep fistula rather to be desired than to be expected.—T.

the rectum, and this union is almost on a level with the perinæum; but in the male, it necessarily happens, that it must proceed much deeper, in order to effect its union with the internal membrane; and by consequence, the operation for fistula is much more easily performed in the female than in the male. The internal coat of the rectum is much more extensive than the muscular one; and their union is effected by an extremely lax cellular structure: hence, when the muscular coat contracts, the internal one is folded on itself, and thus gives the internal surface of the rectum a wrinkled aspect. These wrinkles, which disappear when the rectum is dilated, but which nevertheless are apparent when it is only a little contracted, are all directed transversely, and occupy the three upper parts of the intestine, whereas we rarely find them on its fourth and lower part: we must bear in mind, however, that wrinkles are constantly found here, but that they are always disposed longitudinally.

I have diligently sought, but in vain, for certain semilunar folds which have been stated to have existence here, and more or less in number, their floating edge directed upwards, on the sides of the cavity of the intestine: Morgagni, however, found real valves, instead of these folds, in two subjects. In one of them they had a circular form, but in the other they bore a kind of crossing in their structure. They were situated about the length of the finger above the anus.

We easily see the openings of the mucous ducts along the extent of the internal face of the intestine; but it would be difficult to say whether they are directed in their course from above downwards, or from below upwards; and it is equally difficult to determine that they are simply directed along the longitudinal axis of the intestine.

Mention has also been made of a species of lacunæ, the openings of which are directed upwards, and their bottoms downwards; but I have never been able to discover them. I have, however, always found, at about four or five lines above the margin of the anus, three or four depressions, which bear some resemblance to those which are found at the origin of the aorta and the pulmonary artery, when the sigmoid valves are taken away from them; but those in the rectum are of much greater magnitude: and I have little doubt, but these are what have been mistaken for lacunæ. I have often examined them very attentively, but I never could discover that they presented any opening—I have constantly found them smooth; and can only say that these depressions are generally directed a little upward.

The internal sphincter muscle and the hæmorrhoïdal plexus are found at the lower part of the rectum, between the mucous coat and the fleshy longitudinal fibres. In reality, when the

internal coat of the rectum is dissected away at its lower part, we immediately discover the hæmorrhoïdal plexus, and this said plexus forms a continuation and anastomoses, above, with the internal hæmorrhoïdal veins, below, with the external ones, and in the middle with those which lie between both. The interior surface of the rectum has this hæmorrhoïdal plexus inlaid outside of its internal coat; and when its branches are dilated in this part, the internal coat is marked, indented or impressed, as it were, within side by them; it becomes thin, and takes on a bluish appearance, in such a way that it would seem as if the dilatations projected into the intestine, without any of the proper membrane intervening; in short, the last seems scarcely to have any existence. Nevertheless, if we carefully dissect away the internal or mucous coat of the intestine, we immediately fall in with the individual membrane of the dilated part of the plexus.

On the exterior surface the hæmorrhoïdal plexus is inlaid on the internal sphincter muscle; but it appears to me of importance to remark, that tolerably large branches of the plexus detach themselves, pass through the muscle to its back, and immediately descend on its external face to its lowest edge, and communicate anew, as it were, with the lower border of the hæmorrhoïdal plexus. It thus happens, that the internal sphincter muscle, in persons violently attacked with piles, is traversed, and in a certain degree embraced, by many large veins, so much so as to give it a cavernous aspect. This disposition, however, is scarcely apparent in the dead bodies of persons, who, during their lifetime, had not been troubled with the piles.

It ought further to be remarked, that the hæmorrhoïdal plexus and the veins which give it existence, are more or less dilated and varicous in the immediate neighbourhood of the piles themselves.

I have forced air into the inferior mesenteric vein, by means of the blow-pipe, when the hæmorrhoïdal plexus became distended, and the cellular structure of the inferior portion of the rectum has become emphysematous.

Spirit of turpentine, coloured black, has been thrown into the same vein, and it passed into the hæmorrhoïdal plexus, and it also instantly filled the cellular texture of the margin of the anus.

When the hæmorrhoïdal veins become distended and dilated by the blood, the result is, that they become varicous; but if the blood, by any cause, instead of returning by these veins, descends and diffuses itself at the inferior and internal surface of the anus, into any cells of the cellular structure, communicating with the hæmorrhoïdal veins, the result is a pile: thus,

then, the dilatation of the hæmorrhoïdal veins gives origin to varices, and the blood issuing from these vessels, diffused in a cell of the cellular texture, at the inferior part of the rectum, or at the margin of the anus, is the cause of the pile, properly so called.*

If we dissect the inferior mesenteric vein of a subject having the piles, we find its ramifications to terminate in these pouches of blood. If we detach and completely remove the whole, the piles remain suspended to the branches of the hæmorrhoïdal vein, in a similar manner as the grapes are to the general branch.

These little sanguineous tumours are often the seat of a state of hæmorrhage, and also of violent pain. If they open themselves exteriorly, the blood is evacuated, and the patient relieved till the next attack; but if the piles are seated internally, and they pierce into the rectum, the blood is diffused into it, and passes away by the anus: in this case, however, the excrementitious fluid may drain through the ulcerated orifice, enter into the pile, inflame it, and produce a kind of small depôt, and thus give origin to the fistula in ano. In one hundred cases of fistula of this part, ninety-nine are formed by this procedure, and have their origin from this cause.

After having carefully examined and verified the state of the parts in the large number of dead bodies, which have fallen under my researches during the space of twenty-five years, I have met with seventy-five having had fistula in ano, but who had died of other diseases.

I endeavoured, in the first place, to ascertain correctly, the precise situation where the internal orifice is seated; secondly, the course most ordinarily followed in the march of the fistulæ; and what were the parts through which they travelled.

So far as respects the situation of the internal orifice, I have the most commonly found it a little above the place where the union is effected between the membrane lining the rectum and the external skin; sometimes also a little higher up; but the opening is never seated above five or six lines high; at least in every one of the seventy-five dead subjects in which I have

* That there are two, if no more, species of piles must be well known to any person conversant with the subject; the one is a simple dilatation, but the other is of a firm and fleshy structure, and appears to have been the result of a particular kind of adhesive inflammation, distinctly resembling in its internal action and effects, the pimples which enlarge the end of the nose: but not like this last, inclined to enlarge indefinitely, and heap new formations on those already accomplished. The fleshy pile continues defined in its size, which never becomes large; and after it has existed awhile, it would seem as if the adhesive and accumulative inflammatory action had ceased, and its remains give no farther trouble. The remains of old piles are found near the external verge of the anus; but ofttimes they seem to be simply disused pyriform bags, or shrivelled skin. It would hence appear, that adhesive inflammation contrives to shut up the communication between the internal vessel and the external pile.—T.

found fistula, I have never found one internal orifice to exceed this height, and in a certain number, it was at most only three or four lines up. The opening itself appeared as if ragged or torn; in the greater number it was soft, but hard and callous in a few.

The precise course run through by the fistula was a little variable: in many cases I have seen them, after having taken their origin in the interior of the rectum, descend between its mucous membrane and the internal sphincter muscle. When they have arrived at the inferior part of this muscle, they take a detour and pass between the external sphincter and the skin, and proceed to form an opening in the neighbourhood of the margin of the anus. In many other cases, the fistula passed through the fibres of the internal sphincter muscle, descended immediately between it and the longitudinal fleshy fibres of the rectum, bore down, on the superior face of the external sphincter, passed through its fibres, and then proceeded to ulcerate its way through the skin, at a greater or less distance from the inferior extremity of the rectum. But in a sufficiently large number of the subjects, the parts through which the fistulæ made their way, were so much disorganized and confounded together, that it became impossible to determine their precise course. In all these subjects also, the course of the fistula was lined by a membrane accidentally formed,* extremely thin, smooth and uniform in its texture, and seemingly destitute of any villosity. The result of these researches has convinced me that, in nearly all cases, the fistula is effected in a pile, and sometimes that its passage is determined by some vein of the hæmorrhoïdal plexus.

After these data I think I shall run no risk in affirming, that we ought to be able to discover the internal orifice of the fistula, in most cases, by a single glance of the eye. For this purpose, the patient must be desired to make a slight effort as if to evacuate the bowels, whilst the surgeon at the same mo-

* An opinion has been more commonly entertained, in England, that this kind of lining is the necessary, rather than the accidental, attendant of fistulæ, particularly after they have existed long enough to afford time for its formation: and it was believed, and probably with reason, that an operation stood a much better chance of being successful before this membrane was formed. Were the assertion of our author, that fistulæ are so easily curable, or that they will easily in many cases admit of a spontaneous cure, in every respect correct, we are of opinion that medical men would not so often be consulted respecting fistulæ of five, ten, fifteen or twenty, or more years duration. Perhaps, after all, the fault lies in the deficient experience of the English surgeons in this particular, and this deficiency is rather to be attributed to their want of sufficient opportunities of studying the disease; and to our having no work in our own, or perhaps any other language, which contains any lucid arrangement, or regular and defined account of the disease and its cure, than to any want of attention to, or intelligence respecting the nature of, the subject.—T

ment separates the sides of the anus with his fingers; and, almost always, the little ulceration marking the internal orifice immediately makes its appearance. If we cannot perceive it, it becomes necessary to recur to the probe to discover it with precision; and this means will commonly be successful, except in the hands of an inexpert practitioner.

The idea so generally prevalent, that the internal orifice is very high up, has led the surgeon to direct the end of his probe too high, and the number of attempts to pass it forward, have corresponded with the supposed extent of the denudation. Moreover, the cellular texture of these parts is of so lax a structure, that it is easily, in most cases, penetrated by the stylet or probe; hence, the more easily these instruments pass upwards, the higher up we are disposed to think the internal orifice to be placed: whereas, in that case, it must be a chance indeed if we find it.

The following appears to be the most easy method of following the course of the fistula, and arriving into the rectum with the greatest precision. I have already remarked, that the internal orifices of the fistulæ, are generally situated about the distance of five or six lines above the lowest portion of the intestine, but more commonly less; and that they then march a greater or less distance between the external sphincter muscle and the skin; hence, if we direct the point of the stylet too high, when it is introduced into the fistula, we shall never arrive into the rectum. It will be necessary then, by consequence, in order to accomplish our purpose, to carry the stylet or probe horizontally and almost parallel with the peritonæum, particularly in the female; though, it must be recollected, that in the male it must be directed a little more upward. When we have arrived into the fistula, the button of the stylet or probe must be slid along the internal face of the skin, without ever leaving it; afterwards it must be directed along its lower side towards the lower portion of the rectum; and by this procedure, the internal orifice of the fistula, and the finger introduced into the intestine to meet it, will readily be found. If the patient be placed in a proper position, and we mean to operate at that time by incision, we must gently bring the end of the canulated probe or director, carried through the fistula out of the anus, by giving it a proper degree of curvature; after which we must slide the point of the bistoury along its channelling, with its edge towards the anus, and forthwith divide the teguments from one end of the fistula to the other.

In conclusion I can venture to affirm, that the internal orifice of the fistula is always to be found very near the anus, and at the lowest part of the rectum; and that we are not to operate when the probe will not enter the rectum through an internal orifice; and that there is no need to disquiet ourselves concerning any denudation of the intestine, when the two orifices of the fistula

are included in the incision. In the case of a fistula opening externally, the denudation is never very extensive, nor is the cellular membrane sufficiently dissolved away, that we need despair of a subsequent union of the intestine with the surrounding parts; and that, according to the opinion of Sabatier, we need never in this case divide the intestine.

Baron Larrey first adopted Desault's method of operating on fistula in ano; but having gone through an extensive course of practice, he had frequent opportunities of observing the disease; and also to assure himself that, in almost every case, the internal orifice is found in the lowest portion of the rectum. He had, in consequence, thrown the same light on this operation as he has so brilliantly done on a great number of points in surgery; and he reduced the instrumental part to a straight bistoury, the same as Sabatier. He replaced the ordinary tube by a canulated director, made flexible, and buttoned at the end, which renders the operation extremely easy, and has brought it to the last pitch of perfection.

After what has been said, we see that the fistula in ano is a disease most commonly of little importance, and the operation for its cure is one of the simplest in surgery. The time is passed away when the disease was considered as one of the most serious nature, and the operation as one of the most cruel, and also of the most dangerous cast. This disorder is very common, particularly in persons afflicted with piles; and many labour under it without suspecting it, and are often spontaneously cured. In almost all cases, nothing ought less to disquiet us than the disease, or the operation necessary for its cure.

Whatever be the number of the external openings, they effect no change in the situation of the internal orifice.

F. Ribes.

In taking a review of this paper, two or three remarks present themselves to our consideration.

A general opinion has obtained possession of the minds of medical men, in England at least, that a fistula of any tolerable degree of age, is universally liable to form a membranous lining as a defence to the surrounding cellular membrane, and that this lining must necessarily impede if not prevent a cure. The existence of such lining has perhaps rather been inferred than known to exist from the regular appearance of that lining of the external orifice, which, almost if not always takes place in old fistulæ. The observations of M. Ribes seem to render the existence of such a membrane doubtful, in the generality of cases, though he admits that it does exist in others, where there is great disease of the circumjacent cellular membrane, but he by no means clears

up the circumstance. The existence of this lining membrane of the interior surface of the fistula, has been supposed to be necessary to the welfare of the surrounding parts, and set up by the lax cellular membrane, as a barrier to future sinuous encroachments; as if the cellular membrane were aware of its inability to resist them. Dr. Ribes so seldom mentions any such cases, that it would have been highly gratifying had he spoken expressly on the subject.

We are sufficiently aware that the recent fistula is often and easily cured by incision, but not so well assured that such is the case with the old one. Indeed it is astonishing what a number of old fistulæ are to be met with in London, many of which are of several years duration; and possibly two causes mainly contribute to this continuation of the disease. The English are extremely averse to expose any disorder of this part, and from their not being able to see, either the part affected, or the actions of the surgeon, feeling also a good deal of pain both from the piles which caused it, and the fistula itself when formed, they can rarely be induced even in slight cases to suffer a proper examination of the state of the parts; much less an operation. Further, the lucid acquaintance with the subject displayed by our author, can rarely be in the possession of the great bulk of medical practitioners, and by consequence, betwixt the timidity of the practitioner and that of the patient, the fistula goes on from year to year, and is left entirely to the efforts of nature.

Our author has omitted to make mention whether any fistula, containing the smooth lining he speaks of, were ever submitted to his care in the living subject; and whether there was any greater difficulty in effecting a cure on them, than in other cases. In fistulæ of old standing, the practice of the late Martin Van Butchell deserves notice, though little known as his, at least, either to the public or medical men. His plan was to introduce a hard twisted silken cord, of no great thickness, through the fistula into the intestine, and tie the two ends together on the verge of the anus; as tight as the patient could conveniently bear, and with a slip knot. In four or five days, when the ulcerative inflammation has removed a sufficient portion of the upper edge of the interior opening of the fistula, to slacken the silk; he tightens it again, and so on, from one four or five days to another; till the silken cord finally drops away of itself, when the patient is cured.

The long duration of fistula in ano must either, in the first place, result from the newly formed lining of its interior surface, above alluded to, being the only effort which the parts are able to effect, but which do not amount to a cure, and with which they are content, as being for the present, at least, defended against any farther encroachment. In the second, from the

diseased, and as it were dissolved state of the circumjacent cellular texture, not enabling them to set up and carry on healthy actions and granulations; or, in the third, from the surrounding cellular texture, though not greatly diseased, being unable, from some cause or other, possibly enfeebled action, in a set of vessels and cellular structure, possessed of little of the principles of increased action, to set up and support healthy granulations, whereby a general union of the parts affected with the disease may be completed. Under such circumstances, in old fistulæ, the cure is delayed by want of increased adhesive inflammatory action, and its consequent effect,in deep wounds at least, granulations. This want of action can also prevent or impede the rapid and healthy growth of the proper uniting granulations, even in a fistula which has been operated on: and the slowly resulting cure of which, has given great cause of complaint on the part of the patients. If all these cases be considered, can we conceive a more likely method of raising the granulatory action than the long but gently continued stimulus of Martin Van Butchell's silken cord. If we recollect right, silver wires were used in this way by the earlier surgeons, but to these there are perhaps many objections, as they may not excite so much inflammation as a cord of silk. The practice deserves trying in old fistulæ.

The author has not at all mentioned a kind of idiopathic fistula, which takes place in the cellular membrane, at a more or less remote distance from the rectum; and which appears to be the result of an attack of fever. Possibly some state of the hæmorrhoïdal vessels may give cause to this particular part being chosen to bear the inflammatory brunt, which so commonly attends the first days of common fever; but sometimes it is situated at such a distance as to lead one to suspect that the hæmorrhoïdal vessels have little to do with it; and besides, in such cases, after the breaking of the phlegmon and discharge of its contained matter, the probe passes directly forward in the line of the orifice, not having, to appearance, any necessity to turn, either to the right or left.

The course of such complaint is this: On an attack of fever, pain is felt nearer or farther from the rectum; the inflammation is evidently of the phlegmonous cast, it speedily enlarges and extends inwards; a projection soon takes place externally, and by the time it has arisen to such a size, as to lead one to suppose, from its external appearance, that the whole may be the size of a pullet's egg, the skin becomes thin and bursts. The matter, at first of a good consistency, in the course of days or weeks becomes sanious; the whole interior becomes so contracted as scarcely to admit a probe; the external orifice appears as if lined with a peculiar and newly formed membrane; the

disease becomes more or less quiescent, but every now and then an external healing of the external orifice takes place, and a fresh collection of matter arises; which though seldom of any quantity, say that the whole may or may not fill a tea-spoon, yet pursues the same course as its prototype, and thus the disease continues exhibiting successive attacks for years. Or otherwise, if not so, new ones form, which, from not readily seeing the parts, the patient may not accurately distinguish one from the other; but most probably it is the old one which thus fills and breaks, and so torments him for a long time. In a case where this formation is rather remote from the intestine, will incision of any kind, or any dilatation, be of service? Do these effect, or have they effected, a cure?—T.

The opinion of Dr. Ribes, on the particulars spoken of in our remarks, would be esteemed a treat; and should they fall into his hands, we hope he will so favour us as to communicate his thoughts thereon to our work, for the inspection of our readers.—Ed.

Selected Reviews.

Observations on those Diseases of Females which are attended by Discharges. Illustrated by Copper-plates of the Discharges. By Charles Mansfield Clarke, Member of the Royal College of Surgeons, London. Part II. 8vo. pp. 242. London, 1821.

(From the Medico-Chirurgical Review.)

Concluded from page 88.

To those who have properly considered the nature and objects of this Journal, it will be unnecessary to give any reason why our analyses are sometimes carried to an unusual length, as compared with similar articles in other periodicals. It is quite out of the question that we can compete with our swift-footed monthly cotemporaries, in bringing the *earliest samples* of medical lore into the literary market. It is the object and office of these *avant-couriers* to whet the appetites of the reading public—it is ours to lay more substantial fare before them—and to constitute a medium between the ponderous octavo itself, and the multiform critiques that "spread their light wings," and waft its condemnation or praises through the medical republic. It will always be our anxious wish to proportion our reviews to the value and importance of the works reviewed, though we cannot hope, in so doing, to give entire satisfaction

to all parties—a consummation that, however devoutly it is to be wished, can never be rationally expected in this sublunary scene. We shall do what we think best, and appeal to old CHRONOS for judgment.

In our first analysis of Mr. Clarke's work, we concluded the subject of "Watery Discharge." We now come to the third and last chapter, which treats of—"the Purulent Discharge." The characters of this, are "a heavy, yellowish, opake fluid, possessing little tenacity." Though small in quantity, as compared with the watery discharge, it is far more exhausting to the constitution. Pus may be secreted from membranes without breach of surface, and is then unmixed with blood, which appears, after any violence, where the discharge is from an ulcerated surface—a tolerably fair criterion, Mr. C. thinks, as to whether the pus is secreted by a membrane in a state of inflammation or ulceration. But as a nice distinction of these cases is not always easy, our author first points out those cases of purulent discharge, appearing to arise from the female mucous membranes in a state of inflammation, and, secondly, those other cases, where the pus proceeds from an ulcerated surface.

The mucous membranes lining the vagina, the uterus, and the fallopian tubes, are very differently affected during inflammation. In the two latter, coagulating lymph is generally extravasated, when the inflammatory action runs high. In painful menstruation, (inflammation of the mucous membrane of the uterus) flakes of coagulating lymph are almost always thrown off—in some cases forming accurate casts of the cavity; and this has happened in the fallopian tube also. Sometimes, however, the uterine mucous membrane will secrete pus, and this being discharged *per vaginam*, will render it doubtful whence it proceeds. At other times, the purulent secretion will be retained in utero, in consequence of adhesive inflammation of the cervix uteri. Coagulable lymph is rarely the product of vaginal inflammation—pus is produced by a very slight phlogosis there.

I. *Purulent Secretion from the Uterine Lining.* When this finds its way readily into the vagina, there will be few other symptoms than a sense of heat and uneasiness in the passages. In some cases, where the vagina is wholly free from inflammation, the patient experiences acute pain in the back and bottom of the abdomen, which being severe and constant, the practitioner examines, and finds the uterus tender to the touch, its size being also increased, resembling the viscus when impregnated. The uterus enlarging, the case is still doubtful, till, suddenly, a large quantity of offensive pus escapes, and relief immediately follows. It is no wonder that an unfavourable prognosis is sometimes made in these cases, the practitioner fearing (and with reason) that some morbid alteration of struc-

ture is taking place in the uterus. Two or three cases are here related in illustration. In one case, the uterus burst and discharged the matter into the abdomen, death being the necessary result. In the other case, the matter found its way into the rectum, and was voided by stool, with ultimate recovery.

Treatment. The symptoms will naturally point to the proper treatment—which is that employed in removing inflammation. Cup the sacral region—or apply leeches to the groins once a week—use the hip-bath night and morning—throw warm water up the vagina—give opium or other narcotics to diminish sensibility, while purgatives are to be employed to prevent constipation, and lessen inflammatory action. If the uterus attain the size of the fourth month of pregnancy, it may be presumed that the disease is *not* carcinoma—no tumour of this kind having ever attained such a size in our author's experience. The rapidity of purulent enlargement of the uterus, comparatively with that of fleshy tubercle, will throw some light on the diagnosis; and if there be reason to suppose the enlargement depends on purulent accumulation, "it may be adviseable, gently to introduce the extremity of a bougie, or of a male catheter, into the os uteri, and to pass it onwards, until it has reached the cavity of the uterus."

II. *Vaginal Inflammation.* The symptoms attending common and specific inflammation of the vaginal membrane are nearly the same, and are pretty well known. Attempts indeed, have been made to distinguish between the purulent discharge of gonorrhœa and of common inflammation; but such pretensions are generally professed for self-interested purposes. The lymphatic glands of the groin, Mr. Clarke thinks, are more disposed to enlarge and suppurate in gonorrhœal, than common inflammation.

"In simple inflammation of the mucous membrane of the vagina, the purulent discharge being established in large quantity, the inflammatory symptoms frequently subside very rapidly, after which a termination is put to the secretion; the parts returning to a state of health, provided there be no acting cause producing its continuance; in which case the symptoms will continue until its removal, when they will speedily cease.

"In the case arising from specific contagion, the duration of the disease is greater; and the discharge, once established, continues for weeks, or perhaps for months, although not always accompanied by the other local symptoms." 169.

Mr. C. justly remarks that, it is impossible to determine at what period of the disease, the power of communicating infection ceases. A prudent practitioner will be careful to give no decisive opinion on the subject, especially as regards the non-existence of this power—for, as Mr. Clarke properly ob-

serves, "no person is secure from danger, who indulges in intercourse with a woman so long as the discharge continues."

"It is a curious fact, that in young subjects, both male and female, purulent discharge from the urethra and from the vagina takes place in consequence of the existence of irritation in distant parts; thus, during dentition, whilst the capsule of the tooth, or the gum covering it, is violently pressed upon by the crown of the tooth, the above circumstance is not unusual; medical men, therefore, should be careful to avoid denominating this symptom venereal; since, were it actually so, it would lead to nothing useful in the treatment; and discussions, highly destructive of the peace of families, might be thereby introduced." 170.

Mr. Clarke leaves, of course, the consideration of gonorrhœal inflammation to those writers who have made it the subject of distinct monographs; but, he gives it as his opinion, en passant, that the treatment of venereal gonorrhœa, differs little, if at all, from that which is applicable to common inflammation. As far as temperance, rest, and antiphlogistic measures are concerned, this may be true; but, would bals. copaiba and cubebs be applicable to *common* inflammation of the lining membrane of the urethra?—We should not like to try the experiment.

When the inflammatory symptoms have subsided, and a purulent discharge continues from relaxation or mal-habit of secretion, then our author recommends the cinchona, and copaiva; with astringent injections. Respecting the possibility of secondary symptoms following gonorrhœa, our author seems undecided—at least, he declines to offer an opinion upon the subject.

"He has certainly seen copper-coloured spots on the bodies of patients, who have laboured under gonorrhœa virulenta, removable only by the oxymuriate of mercury and sarsaparilla: but he thinks that he has seen similar appearances upon the skin of patients, whose chastity could not be suspected." 173.

Mr. Clarke has known several instances of married women, who had laboured under a purulent discharge, (possibly the effect of contagion,) bringing forth children prematurely, some dead, and others having dark-coloured furfuraceous cuticle in different parts of the body, which yielded only to mild preparations of mercury.

Modern experience has put the matter quite beyond a doubt, that secondary symptoms will occasionally follow gonorrhœa; but the chances are so much against the accident, that we should never dream of giving mercury as a preventive. Indeed, we are disposed to believe that, were mercury given as a preventive, there would be more secondary symptoms than if omitted, in consequence of imprudence in diet, and exposure to wet or cold, which, it is well known, are very injurious where mercury is in administration.

Abscesses sometimes occur in the cellular membrane surrounding the vagina, and do not admit of the same modes of cure, that are applicable to other purulent discharges. Such cases, however, are very rare. At the commencement of the complaint, there is nothing to discriminate it. Inflammatory symptoms are followed by a discharge of matter, and the patient supposes herself well. But after some time, a sense of fulness and pressure is experienced, and pus again escapes. On examination, a soft tumour will be found behind the vagina, which, being pressed, discharges pus. A continuance of this state impairs the health, and the neighbouring parts become more than usually irritable. Indeed, it has only occurred in women of lax fibre and irritable constitution. In cases of this kind, the abscess generally breaks into the vagina high up, which aggravates the inconvenience, the abscess being seldom emptied of its contents, which become, from retention, highly offensive. These cases are very unmanageable. No astringents can restrain the discharge—no stimulants can reach the cavity of the abscess—and it is hazardous to attempt an operation, unless the most depending part of the abscess should be situated so low, as to be capable of being brought within sight of the surgeon. Mr. Clarke relates three cases. In two of them, there was a cure effected by remedies adapted to improve the general health. In the third, he failed to remove the disease.

III. *Ulceration of the Os and Cervix Uteri.* Our author has long been in the habit of describing two different kinds of ulceration of the uterus—the one denominated the "corroding," the other, the "carcinomatous" ulcer of the os uteri. They are both malign.

Corroding Ulcer. This disease usually takes place about the cessation of the catamenia. About this period, the uterus naturally attains a larger size than ordinary, and is not reduced to its usual volume, until the balance of the constitution is restored.

Inflammation attacking a part of firm texture, as the os uteri, an extravasation of coagulable lymph is more frequently formed than abscess, occasioning a thickening of the part. Sometimes this proceeds to ulceration—sometimes the inflammation recedes, leaving only an induration of the part.

"In the corroding ulcer of the os uteri, the membrane which covers it first takes on disease, and very shortly afterwards the ulcer extends to the whole circumference of the opening, and to the parts immediately beneath it; so that the natural shape of the os uteri is destroyed. Thence the ulceration proceeds to the cervix, and consumes it; so that, if the patient should die in this stage of the disease, nothing will be found, after death, but the body and the fundus

of the uterus. Sometimes the disease does not stop here, but, before the patient is destroyed, the absorbents employed in the process of ulceration will have taken up nearly the whole body of the uterus, so that very little more than the fundus will remain." 188.

This does not happen in the carcinomatous ulcer, by which the patient is worn down before there is time for such a degree of absorption to take place. "If an examination be made, per vaginam, the breach of surface may be readily distinguished, and the extent of the disease ascertained; but no hardness of the parts will be present, no thickening, no deposit of new matter." The appearances, post mortem, will correspond. From our excellent author's symptomatology, we shall make the following extract.

"The menstruous secretion, it has been already said, has ceased; in its stead a yellowish discharge escapes, perhaps trifling in quantity, and now and then mixed with a streak of blood; by degrees the sense of warmth is converted into a glowing heat, affecting the region of the uterus; and it is by no means uncommon with patients to state, that they feel 'as if a hot coal was within them.'

"As this sensation of heat increases, so the quantity of the discharge increases, the ulceration extending more rapidly.

"The perpetual drain necessarily diminishes the quantity of circulating blood; in consequence of which the countenance becomes pallid, and weakness of the whole system is produced." 191.

Had the disease been carcinomatous ulceration, lancinating pain would have been the most prominent feature of the complaint. But this distressing symptom is comparatively absent in the ulceration under discussion.

"When a finger, introduced into the vagina, is made to pass over the ulceration, the patient does not complain of pain; she does not suddenly shrink from pressure, as when carcinomatous ulceration is present: but if asked what sensation she experiences, she will commonly reply, that she has a sense of soreness." 192.

Still the disease in question is as uniformly fatal as carcinoma uteri; but it will last during a much longer time, unless the fatal termination is hastened by hæmorrhage.

Treatment.—Although the uterus performs no office in the constitution when menstruation has ceased, yet it is liable to morbid changes, that too often involve the constitution in destruction. Ulceration having once commenced in the uterus, this organ never, in our author's experience, recovers its healthy structure; "but increased action of the blood-vessels of the os uteri, which would eventually terminate in ulceration, may probably be diminished or controlled, so that no ulceration may

take place, and, by such a mode of treatment, much advantage may be gained."

"Whenever, therefore, a patient in whom the menstruous secretion has recently ceased, complains of an increase of heat in the lower part of the back, or of the abdomen, or in the parts of generation themselves, a prudent practitioner, foreseeing the probable result, will direct the loss of some blood from the neighbouring parts. The most precise mode of obtaining this blood will be by cupping; although, if the patient be averse to the operation, leeches may be applied; but, upon the whole, they do not afford the same certain and immediate relief, neither can the quantity of blood removed by them be so exactly estimated." 194.

The operation should be repeated at the termination of a fortnight or three weeks, and if the sense of heat should continue, a still further loss should be directed—since temporary weakness is the only disadvantage which can accrue. General bleedings are not proper of course. The warm hip-bath twice a day is highly serviceable, and some of the warm water may be thrown up the vagina—both processes are equally beneficial in the indurated and ulcerated stages of the complaint.

"Saline purgatives, exhibited in very small doses, possess not only the power of allaying inflammation, by the watery secretions which they produce from the intestines, but they appear also to possess a specific power of tranquillizing the system, when in a state of disturbance and increased action, even when they produce very little sensible effect." 196.

Small doses of the sulphas magnesiæ vel potassæ may be exhibited twice a day, in combination with moderate doses of conium or hyoscyamus. Mr. Clarke does not object to the use of sarsaparilla or other alterative, provided such medicines do not derange the functions of the stomach, or impair the powers of the constitution. An abstemious diet should be enjoined, as fish, puddings, boiled fruits, and vegetables; and all excitants religiously avoided.

Even in the second stage, when ulceration is proceeding, and the patient is already weakened by the purulent discharge, what supports that ulceration but the inflammatory process? What remedies are likely to be more serviceable than those which retard it? It at length, however, happens that the patient becomes so debilitated by the purulent secretion that she is in danger of sinking from its effects. Mild astringent injections must then be thrown up into the vagina. Hæmorrhage may arise, and then still stronger applications are necessary, as alum in decoct. querci vel granati. If this should fail, solutions of sulphate of copper, or even nitrate of silver must be employed. The horizontal position—and small doses of the acidum sulphuricum in

a mixture containing equal parts of decoct. cinchonæ and infus. rosæ, will be found proper auxiliaries.

IV. *Ulcerated Carcinoma of the Rectum.* The vicinity and sympathy between the rectum and uterus, will render it sometimes difficult to determine which part is affected, without an accurate inquiry and examination. The uterus is *more* liable to cancer than the rectum—and this should be borne in mind. In the early stage of carcinoma recti there is a mucous discharge, which gradually becomes purulent, and this appearance may lead us to suspect fistula, but examination puts the question to rest.

"If the finger of the practitioner be carried into the rectum, it will be girt by a constriction of considerable thickness, through which it cannot be passed; and if any attempt is made to surmount the difficulty by violence, the patient will suffer excruciating pain, and a discharge of blood will be the consequence of such a rude inquiry." 203.

It is useless to be too anxious to ascertain the extent of the disease. A small carcinomatous thickening of the intestinal canal is as fatal as that of a larger portion. A carcinoma affecting not more than a quarter of an inch of the rectum may, by obstructing the passage of the fæces, cause a distention of the whole colon, and fatal inflammation, the consequence of that distention—a carcinoma of great extent can do no more.

In *common* ulceration the part is absorbed—in *carcinomatous*, new matter is deposited, as the old is removed, and thus the thickening and destructive processes proceed simultaneously. It is evident that the rectum cannot be kept at rest; and if the constipation be not viewed in relation to its proper cause, and the patient be exposed to the action of frequent purgatives, the symptoms will be aggravated by the means employed to alleviate them. The following melancholy picture is not overcharged, as we have too often had opportunities of verifying its correctness.

"All the symptoms which attend the first stage of this disease will be found to exist in a greater degree in the second. The darting pain will be increased both in frequency and in violence; the action of the heart will be greatly and permanently accelerated; the functions of the stomach will become more and more impaired; vomiting will be almost constantly present; temporary relief will be found only in opium; and permanent rest only in the grave. In the progress of the ulceration, blood-vessels will be exposed which will pour out, according to their size, a larger or a smaller quantity of blood: and happy would it be for the patient if such hæmorrhage should prove fatal; but such an event is hardly to be expected; and, unless in parts more

abundantly supplied with blood than the rectum, such an occurrence is seldom met with." 207.

As nearly the same treatment is necessary in this case as in ulcerated carcinoma of the uterus, we shall proceed at once to the latter subject.

V. *Ulcerated Carcinoma Uteri.* The carcinomatous *tumour* of the cervix uteri has been treated of in our author's former volume, to which we must refer the reader. A woman may live many years under such circumstances, provided she submits to proper rules; but sometimes our best efforts fail, and the ulcerative process takes place, on a more or less extended surface. In the latter case, the disease will be tedious—in the *former*, the fatal termination will be rapid. It ought to be borne in mind, however, that the ulceration goes on more rapidly at first, than in the progress of the disease—owing probably to weakened power of absorption in the latter period, keeping a ratio with the general decline of the powers of life.

"Thus a number of instances will be found, in which the patient will exist in a state of extreme weakness during many weeks or even months, contrary to the expectations of the medical attendant. Spontaneous bleedings from the ulcerated surface, producing more sudden debility, will have the same effect in retarding the progress of the disorder." 212.

In an early stage of the ulceration, it is not unusual for the patient to complain of a puffy and enlarged state of the external organs—owing to the increased action of the neighbouring vessels. A great degree of itching is another symptom, and sometimes erysipelas. The cuticle often desquamates—trifling oozing ensues, which, drying on the surface, forms furfuraceous scales, a new source of irritation, often extending to the groins and insides of the thighs. The discharge, at first ichorous, and afterwards purulent, is by no means comparable in quantity with that which is met with in some other diseases of those organs—and sometimes even diminishes as the disease advances, in consequence of the diminished quantity of blood in circulation.

If the bladder and rectum have not suffered in the early stage, they seldom escape now—not only from sympathy, but from the disease extending itself to these, in common with the adjacent parts. Such a degree of thickening takes place sometimes in the meatus urinarius as to impede the passage of the urine, and require the catheter—shortly after which the urine will spontaneously escape, not through its urinary passage, but through a communication between the neck of the bladder and vagina.

In a very few cases a communication between the rectum and vagina takes place, and from that moment no fæces pass by the anus—the external parts forming the channel through which urine, fæces, and pus are discharged. The stench now becomes intolerable, and the hips of the patient lying immersed in the excreted matters, the soft parts inflame, and sloughing takes place! It is needless to say that the wear and tear of the constitution, under such circumstances, is great. The patient becomes a skeleton, and dies under a complication of the most terrible phenomena which it is the lot of hapless mortality to endure! The situation of a woman labouring under carcinoma uteri is infinitely more pitiable than that of one with cancer in the breast; for not only are the symptoms more numerous and insufferable, but she has not the good fortune to be cut off, in the progress of the disease, by accidental occurrences. In mammary carcinoma the patient is usually destroyed by hydrothorax—but no such blessing is afforded to the subject of carcinoma uteri; the sufferer being compelled to endure till her frame is exhausted by pain, by vomiting, by want of sleep, by discharge, by an offensive atmosphere, or by gangrene of the integuments!

VI. *Treatment.* Though carcinomatous ulceration does not obey the laws by which common inflammation is governed, it is nevertheless controlled, in some measure, by those means which subdue common inflammation.

"Whenever a patient, labouring under carcinoma of the uterus, has placed herself under the constant care of a physician or surgeon, it will be necessary that he should watch with attention the changes which take place in her constitution. If he should find the circulation becoming accelerated, the skin more than usually hot, flabbiness of the integuments, softness and shrinking of the muscles in different parts of the body, he may presume that some important change has taken place in the diseased organ. If, together with these symptoms, the lancinating pain has been rendered more acute; if the sympathies between the uterus and the adjacent parts, or between that organ and the stomach, have been more than usually called forth; or if, lastly, the mucous discharge has assumed a puriform character, there can be little doubt that a breach of surface has taken place, and that the complaint has acquired its most frightful and distressing character." 219.

If the patient possess a vigorous constitution, we are hardly authorized to omit general bleeding; but this will seldom be requisite, as local bleedings can be repeated as often as necessary. The quantity to be abstracted in this way may vary from six to twelve ounces at a time, and the glasses should be applied just above the fissure between the nates. If it be judged proper

to bleed from the lower part of the abdomen, leeches should be scattered above the pubes, from one groin to the other. In the progress of the complaint leeches may be applied to the labia, or even within the vestibulum, "by means of which more relief is sometimes obtained than by their application to the pubis." Local blood-letting should be employed once in three weeks or a month—provided the patient be not weakened by it, or exhausted by pain or discharge. Spontaneous hæmorrhage, to the extent of syncope, not unfrequently arrests for some time the progress of the disease. Even in the latter stages, when the loss of blood might appear unwarrantable, it may still be proper to recommend it; since it is well known that carcinoma uteri, in its ulcerative stage, involves all the neighbouring parts in a state of inflammation and irritation. For instance, when the rectum is attacked, there is tenesmus, great heat in that part, increased distress in voiding the fæces, exquisite tenderness of the gut, if the finger be carried into it. So in like manner, if the disease proceeds to the bladder, shivering usually comes on, succeeded by great pain, and strangury. If the disease makes its way, which is not very common, into the abdomen, symptoms of peritoneal inflammation will, of course, make their appearance. All these circumstances, therefore, call for the loss of blood, even in a late period of the disease.

The management of the discharge is a matter of great importance; for there is reason to believe that the spreading of carcinomatous ulcerations may be greatly retarded by the absorption or removal of the ichorous fluid secreted by them.

"Of all the modes of applying water to sores at the upper part of the vagina, none is so effectual as the use of the hip-bath; in the employment of which, the water is brought into contact with the sore without any risk of injuring the latter. By these means, the object of maintaining cleanliness is not only obtained, but a soothing application is made to an irritable surface; the careful injection of warm water into the vagina, by a syringe, or the agitation of the water by the hand, will render it more likely to remove any portions of coagulating lymph or thickened matter which may adhere to the inside of the vagina. The heat of the water employed should depend upon the feelings of the patient in some measure; but, generally speaking, it may vary from about 86° to 94°. Where the patient is too weak to bear the exertion of being placed in a hip-bath, her hips may be brought over the edge of the bed, and warm water may be carefully injected into the vagina by a female syringe. The quantity of the discharge is frequently increased by the means abovementioned, but the comfort which the patient will derive from it will abundantly compensate her for any debility which may be produced by the remedy; and excruciating attacks of pain are sometimes rendered very sufferable by a frequent recurrence to it. Strong decoction of carrots, sometimes used for the same purpose, has the happiest effects. Warm

water may also be made the vehicle for a variety of sedative applications, which are found by experience to tranquillize all irritable sores; and, in some, to expedite the healing process. Amongst the different applications for this purpose, the extractum conii, or extractum hyoscyami, may be mentioned, either of which may be employed in the proportion of about three or four drachms to a pint of water. Solutions of opium, or of extract of poppy, may also be used; of the former, two drachms; of the latter, half an ounce, may be dissolved in each pint of water. Starch, or mucilage of quince-seed, form good menstrua for these applications; their adhesive property enabling them to cling to surfaces to which they are applied. Three or four ounces of either of these fluids, impregnated with sedative substances, may be thrown into the rectum in those cases where relief is not obtained by their application to the vagina; but when opium is used for this purpose, the practitioner should be very careful to watch over its effects, as it has sometimes happened that unpleasant consequences have arisen from the application of this drug to the rectum, such as vomiting, syncope, cold extremities, and irregularity of the circulation. The action of the absorbents of the rectum is, in all probability, in these cases, increased by the inflammatory process which exists in the vicinity; besides which, the action of the rectum itself is temporarily taken off, so that the enema will probably be retained during a considerable length of time. Plasters and liniments, into the composition of which opium enters largely, will sometimes be found serviceable in allaying pain, and are useful auxiliaries in a disease in which all the resources of the practitioner may be required to diminish the sufferings of the patient." 229.

A dilute mixture of acetic or nitric acid in water, forms a soothing application to an irritable part, as acidi acetici ℥ss. aquæ distillatæ oj. m. ft. injectio. Or, acidi nitrici gutt. x. aquæ distillatæ oj. m. fiat injectio. Some liquor plumbi acetatis may be added to either of the above. If the discharge become so profuse as to cause debility, astringent injections must be employed: as decoct. cort. granati oj. sulphatis aluminis ℥ss. m. ft. injectio. Or, half a drachm of sulphate of zinc, fifteen ounces of water, and an ounce of tincture of kino, make a useful injection. If hæmorrhage occur, and the patient be in great pain at the time, it may be right *not* to restrain it hastily, unless the patient's strength has been previously much exhausted. When it is desirable to restrain the flow of blood, the foregoing injections may suffice; or ten grains of argenti nitras to a pint of distilled water.

As to internal remedies, pain being the symptom most complained of by the patient, the sedative class of medicines form our sheet anchor. But, as our author justly observes, we should never exhibit a medicine of strong sedative power, when a milder will produce equal relief—since the disease is of long duration, and every medicine will at last fail of producing its effect. Hyoscyamus and conium may be first employed, and afterwards

we must have recourse to stramonium, belladonna, and opium. In some constitutions the solid opium, in others the liquid form will agree best—some prefer liq. opii sedativus—others, the black drop—in short, he will be the best practitioner who best understands the adaptation of these means to their end, and who can smooth the passage of the afflicted patient from this to another, and, we hope, a better world!

We have now turned over the last page of Mr. Clarke's work, and take our leave of him, perhaps for ever. We implicitly believe the concluding assertion.

"In thus concluding his work, the writer can conscientiously assert, that he has made no statements which, in his opinion, are not founded in fact, and that he has withheld nothing which might in any way tend to the advantage of the practitioner, or to the comfort of the patient." 237.

The language is plain and perspicuous, and Mr. Clarke has the art of embodying his ideas in the most distinct and tangible shapes—an art, of all others, the most useful and difficult. To us, who are plain practical men, the work appears highly valuable; and we leave it to our readers to form their own conclusions from the ample specimens which we have now laid before them.

Experimental Observations on the Operation of Lithotomy, with the Description of a Fascia of the Prostate Gland, which appears to explain Anatomically the Cause of Urinal Infiltrations, and Consequent Death. By Granville Sharp Pattison, Esq. Surgeon.—From the American Medical Recorder for January, 1820.

Observations on the Parts concerned in Lithotomy, which are intended to prove that Mr. Pattison's Ideas of a Prostate Fascia are erroneous. By Horatio G. Jameson, of Baltimore, M. D.—American Medical Recorder, July, 1820.

A Reply to certain Oral and Written Criticisms, delivered against an Essay on Lithotomy, published in the January Number of the American Medical Recorder. By Granville Sharp Pattison, Esq.

(From the Quarterly Journal of Foreign Medicine and Surgery.)

What can be more interesting to Surgeons than the question involved in the papers at the head of this article? An inquiry into the cause of the many deaths which occur in consequence of the operation of lithotomy. It would be a mere waste of words to argue upon the importance of such an investigation.

In these Essays there is displayed a little of that rancour which generally insinuates itself into the discussion of surgical subjects; still it is a happy omen for the surgery of America, that keen disputes have commenced on points of surgical anatomy. The present controversy is upon the merits of the discovery of a fascia connected with the prostate gland. Although the rage of discovering new fasciæ at the groin has subsided, and though many of these discoveries are now forgotten, still it cannot be denied, that much practical good resulted from the investigation of how far the *discoverers* were entitled to their claims. But both hernia and aneurism have now gone out of fashion; still, in the anatomy of the fasciæ of the perineum, there is as wide a field for contention, and it promises as rich a harvest to *discoverers*, and as great a torment to our students, as the fasciæ connected with hernia did formerly. Yet we may hope that much practical good will result from the critical examination of the anatomy of the pelvis. In this country, at the present moment, the question of lithotomy is by some very keenly taken up, but instead of its being made the matter of anatomical and pathological discussion, it is attempted to be settled by the repetition of experiments on the living body, which, by almost every surgeon, were supposed to be set at rest by the experience of ages. The high operation is again attempted to be introduced into general practice. It only requires this to be said, to prove that those who wish to introduce it, have conceived upon strong conviction that there is something radically wrong in the lateral operation. If the common method of performing the lateral operation is to be with the gorget, then it is not wonderful that surgeons should attempt to change their manner of operating. As it is generally agreed, that in such an important operation as extracting the stone, we must be guided in a great measure by the records of the proportionate success of the different methods of operating, we must here give a slight sketch of the history of the high operation. In the latter part of this Essay, more shall be said on the merits of this operation, and on the manner in which the discussion has again been brought before the public.

History shows us, that the high operation has, in all ages, had warm advocates in the persons of young surgeons; but it also shows us, that those same surgeons, when they had more experience, gave it up, except in very particular cases. It was at one time keenly patronized by Cheselden, but he gave it up in favour of the lateral operation, and almost every author who has written upon it, and particularly Middleton, Smith, Douglas, and Heister, although they were in the first part of their career keen advocates for the operation, state that a very small proportion of patients recover, upon whom this operation has been

performed, and who have passed their thirtieth year. What was the success of Rau and Cheselden? It has been stated, upon the best authority, that Rau cut 1500 patients successfully, and we have been told by an authority which is open to contradiction, if his testimony be not correct, by many gentlemen at present in London, that the professor of surgery in Moscow (Hildenbrand,) at this moment probably alive, had cut previous to 1818, more patients than Rau, and that the proportion of deaths was only one in fifty; but he does not cut with the gorget. The surgeon who gave us this information saw him perform it fifty times, and of those only one patient died. This should be sufficient proof, that the lateral operation, performed in a *certain way*, is a very successful operation; therefore it is of the first moment to inquire what is the best method.*

Let it be recollected, that the present advocates for the high operation recommend the use of the gorget in the lateral operation. It has been already stated, that it is not surprising that those who imagine the gorget to be the best instrument for performing the lateral operation, should be desirous of giving up the operation altogether; and why, may be discovered by the following list of difficulties and mishaps, some of which every surgeon, who has seen many operations with the gorget, must have witnessed. But in the first place, let us inquire why it is that surgeons prefer the operation with the gorget. Is it not because, having the staff in the bladder, into which they can nicely fit the beak of the gorget, that they think there is no fear of missing the bladder, when they force in the instrument. How often does one hear students say "the gorget for me; let me get the beak into the groove of the staff, and then I shall cut into the bladder." This then is the grand secret; but is not the surgeon often foiled in his attempt?—that must be granted; and what have the consequences been? Has not the patient been *frequently* sent to bed with the conviction, at least the assertion, that there is no stone? Has not the patient died, and the bladder been found uncut, and the stone lying in it? Has it not been often, *often* said that the stone is sacculated; and upon examination after death the bladder found to be the sac? Has not a surgeon, determined not to be foiled, brought away the

* We have upon good authority been informed, that there is at present in St. Petersburgh, a young man, whose father was an itinerant lithotomist. The son was taught by the father the method of Frere Jacques, and commenced at the age of fourteen to cut patients. He was so successful, that he got a license from Government, to cut the patients in all the hospitals of St. Petersburgh. The lithotomist is now twenty-two years of age, and he has already cut more patients than any six surgeons in London. We dare not state the number that has been given to us, for it might destroy the credit of the story with many, though we place implicit confidence in the honour of the gentlemen who have reported it to us. But we shall make it our duty to inquire farther into the subject.

stone surrounded by part of the bladder? These are but one train of accidents which, to the knowledge of many, have occurred not unfrequently within the last ten years.

Let us endeavour to give some idea how such accidents may happen. The principal reason is, that the bladder has not been opened, and how does that take place?

1st. Let any one examine the gorgets in the principal cutlers' shops in London. The beak of the gorget is made very small, and the groove in the staff is made to correspond to it, and this instrument is believed to be good, because the two instruments accurately fit each other. The first objection to the instrument is very obvious, if we recollect the size of the urethra compared to the size of the groove, but in operation; it is still more so, when we see the difficulty the surgeon has in finding the staff, and cutting into the groove: how often has a surgeon been seen to make half a dozen cuts into the groove, before he can fix the beak? Is it not natural to suppose, that being foiled in this, which appears simple, he may lose his presence of mind and not fix the beak at last? All this difficulty is owing to the imperfect formation of the staff, an imperfection which ought to be at once apparent to any one who recollects the size of the urethra. On this point we cannot do better than quote the following from the Surgical Observations by Mr. C. Bell, No V. p. 131. "The staff is made too small, it ought to dilate fully, and occupy the urethra; the groove should be wide and deep. The staffs in the shops are small, with a trifling groove adapted for the beak of the gorget, by which surgeons have contrived to make a simple matter difficult. The consequence of the staff being made small, is, that the membrane of the urethra rolls upon it and is cut with difficulty; and when the cut is made, the sides of it do not expand to display the wound, the membrane closes again, and a second and a third wound must be attempted; who has seen much of the operation with the gorget, and has not seen the frequent repetition of incisions into the groove of the staff, and the frequent attempts to lodge the beak of the gorget in the groove of the staff? When we have a staff as large as the largest catheter, and the groove deep and broad, it makes this part of the operation easy; the membrane over the groove is easily struck, and when this is done the wound opens, and is easily found again if it should appear necessary to withdraw the knife before completing the incisions in the bladder." With such a staff it is hardly possible for the greatest bungler to be foiled in getting into the bladder, even with the gorget.

2d. Look to the length of the staffs as they are commonly made, and seeing how short they are, is it not highly probable, that the assistant may so far forget his duty, during the several attempts of the surgeon to introduce the beak into the groove,

as to push the point of the staff through the hole which has been made in the urethra, and thus direct the gorget towards the rectum? The danger of such an accident is easily shown on the dead body. But let us suppose that the beak of the gorget is fairly in the groove of the staff, is there now no danger of its slipping? To show that this may happen, take the gorget and staff in your hand, run the beak along the groove, see how difficult it is to keep it right, even with the eye directing the gorget; then think of the difficulty there will be in directing it along the axis of the pelvis, with the patient struggling, and when in imitation of a master you wish to do it with an air of decision. It may be truly said, to do this well, requires the hand of a master.

The next series of accidents occur at the neck of the bladder; they will not be dwelt on here, for the limits of the paper will not permit of it; but what has been *proved* by dissection to have happened, will be merely mentioned. The bladder has been driven before the gorget, and in groping with the finger it has been separated from its connexion to the os pubis. This was in a child. The bladder has been pushed before the gorget, and a large sac has been formed anterior to the bladder by the motion of the forceps. This was in an adult. In both of these cases the stones were alleged to be contained in *sacs*, until dissection proved the sac was formed by the bladder.

Perhaps this accident will be allowed to be liable to happen, if (the bladder being empty of urine) the beak strike against the stone, and thus carry all before it; or if the prostate be of a scirrhous hardness, such as will produce more resistance than the loose connexions of the neck of the bladder are equal to. Both of these cases may be proved by dissection. The inner membrane of the bladder may be pushed before the gorget. Of this we have no proof by dissection; but a surgeon, who always operates with the knife, observes, that he has felt, on pushing his finger in with the knife, the danger of pushing up the inner membrane. The danger of driving the gorget through the bladder, of wounding the rectum, or the pudic artery, need not be dwelt on. These are faults of the operator, more than of the instruments.

The *facts* which have been stated will be sufficient to prove, that the operation with the gorget is not so simple an operation as some would pretend, when they recommend the gorget as the best instrument "*for the man who is not well acquainted with the anatomy of the parts.*" One may be allowed, in thinking of such a sentiment, to express himself warmly. Can any good man read the cases which are detailed, or think of the difficulties which the greatest characters in the profession of surgery have acknowledged they have felt, in making up their

minds as to what parts are to be cut; can he bring himself to perform such an operation as that of lithotomy, conscious that he must trust *all* to the adaptation of his instruments; and fully aware that if he makes the slightest slip, that he is so ignorant of the anatomy of the parts that he cannot remedy his mistake. The law has been of late severe in punishing presuming ignorance with heavy penalties; but what is a crooked arm to the crime of causing the death of a man, and that by greater tortures than were ever invented by the fiercest bigot. The very idea of a man unacquainted with the anatomy of the perineum, undertaking the operation of lithotomy, makes one shudder.

But there is really a great mistake in supposing that the gorget is the easiest instrument to pass into the bladder; and of this we hope our reader will be satisfied when he compares the operation by the knife with that of the gorget.

A large grooved staff, such as has been already described, and which will distend the urethra to the utmost, is fairly lodged in the bladder. A deep and a cautious incision, one perfectly safe to those who know the anatomy, is to be made through the muscles, fat, &c., by the side of the anus. Now the staff can be easily felt, and easily cut upon, and the urethra being put upon the stretch, by the wideness of the groove, gapes open, when an incision is made into it, instead of closing again, as in the common operation with the gorget. Now, where is the difficulty of carrying the same knife that has made the outer incision, or a common bistoury, into the bladder; or where is the danger of cutting any thing but what we wish, if we place the forefinger of the left hand upon the part of the staff into which the knife has entered, and push the knife forward with the right hand, keeping it correctly in its place with the left forefinger, until the knife cuts a *way* for this finger into the bladder; *now*, the finger is in the bladder, the stone can be felt, and all our fears of not reaching the bladder should be at an end; we may enlarge the opening in the bladder, and that as we will, with the same knife, or with a probe bistoury. Here then is a *feeling* operation most essentially different from the random plunge of the gorget.

Let us here quote the description of the operation with the knife from the work of the author, who has the merit of *always* performing this operation, and of having been on every occasion the strenuous adviser of it.

"1st. The staff is struck upon the stone, and made to rest in contact with it.

"2d. The point of the knife is struck into the left side of the perineum, just under the arch of the pubes, it is carried downwards by the side of the anus, and past it.

"3d. The forefinger of the left hand is put into the wound,

and the rectum pressed down, and the muscles and internal fascia cut across.

"4th. The edge of the knife is turned up, and the membranous part of the urethra, and the face of the prostate gland exposed.

"5th. The point of the knife is pushed into the groove of the staff, and carried forward, and the prostate gland is opened by a lateral movement of the knife.

"6th. The forefinger of the left hand follows the knife. The edge of the knife is directed by the forefinger, and enlarges the wound of the bladder; the knife is withdrawn, and the forefinger is thrust deeper, so as to touch the stone.

"7th. The forceps are introduced, and touch the stone."

It has been said, that the operation with the knife must be very difficult to perform in a fat man; but in saying so, it is forgotten, that all the fat is external to the prostate, and that through this fat we must make such an incision as will admit the three fingers knuckle deep; to say that the gorget is the best instrument for a fat patient, is only saying, that the farther the cutting point is from the part held in the hand, the easier is the cut made, or the more concealed the part, the more boldly we make the plunge.

Let us now consider the objections which have been made to the lateral operation, by those who use the gorget in preference to the knife. The great danger is supposed to be from bleeding, but the means hinted at, to prevent this accident, militate more against the plan of cutting certain parts, than against the instrument. Upon the authority of Mr. Pattison, we state the following conversation, which he says he had with Mr. A. Cooper. It is proper to remark, that in Mr. Pattison's first Essay he gives the following conversation, as one he had "with my friend Mr.——, who deservedly stands at the very head of his profession in Great Britain." But in defence of his paper he gives us Mr. Cooper's name. "The longer I practise, the more am I convinced, that the smaller the wound made in operating for lithotomy the better.* I may not live, but probably you will, to

* In answer to this we state the following experiment:—The pelvis was divided so as to leave the bladder and penis attached to the right side. The parts were so dissected as to leave the prostate and bladder quite exposed on their left side. A stone was put in by the fundus of the bladder; an opening was made in the membranous part of the urethra and prostate, sufficient to admit the finger and a pair of forceps. The stone was about an inch in diameter. It required the whole force of one person to keep the portion of the body on the table, while another was trying to drag the stone through the small opening; and it was also necessary, before the stone could be extracted, to push with the left hand against the face of the prostate, otherwise the whole would have been pulled out with the stone. This may give an idea of the resistance of the parts at the neck of the bladder, which we are advised to dilate: for it should be recollected, that the arch of the bones could not resist here, as

see the neglected and despised Marian method (apparatus major) under some slight modification revived. I do not conceive that the danger of a large wound arises from the division of a membranous part, but from the risque that in making such, some large vessel will be wounded and much blood lost. Hemorrhage is I conceive the cause of death, in most of the operations which terminate fatally; at least, every patient of mine who has died, has, during the operation, lost a very great quantity of blood."

If one will consider the quantity of blood which is sometimes lost by accident, sometimes after amputation, and that which is lost by flooding, and compare it with the most bloody operation of lithotomy (where the pudica communis is not opened), then we must come to the conclusion, that some other cause for death after lithotomy must be assigned. A patient was said to have died of bleeding: on injecting the pelvis, to discover the source of the supposed fatal hemorrhage, not an artery larger than the transversalis was found opened.* It is not possible to imagine that Mr. Cooper can cut the pudica communis; and as we can have no faith in there being in every fatal case of lithotomy a lusus naturæ in the distribution of the arteries, nor even in the "large arteries on the prostate," which Mr. Carpue makes, by injecting veins with glue, and which he begs leave to inform us he has been in the habit of doing for a number of years, we must suppose that there is generally some other cause of death; and the most probable one, is the irritation caused by the lodgement of urine, added to the general shock produced by the operation.

This is not a new opinion. Scarpa lately wrote a Memoir on the subject, and Mr. Pattison has supposed, that, during his investigation of Scarpa's observations, he has discovered a certain fascia which will explain the cause of the infiltration of urine. As the irritation produced by the lodgement of the urine

the pelvis was divided, neither could the muscles, as they were removed. It was the sphincter of the bladder and the prostate only. On everting the parts, after the stone was brought through, the prostate was not found dilated, but *broken* into many small portions, and the ducts of the testicle quite destroyed. This method of castrating must be performed frequently, where the parts are thus lacerated; but it may be avoided by cutting sufficiently.

* Two years ago we received a communication from a friend in Paris, informing us that, to avoid the danger of bleeding, M. Dupuytren had invented a new operation. He cut the bladder above the prostate. We recollect, at that time, that in dissecting a subject we found the obturator artery give off the pudic, which clearly demonstrated, that even this new form of incision was not free of the supposed danger. We had forgotten this plan of M. Dupuytren's until a few days ago, when conversing with a very intelligent foreigner on the subject of lithotomy, he told us, that he had seen M. Dupuytren cut seven patients in this manner; of those, five died in consequence of lodgement of the urine between the bladder and pubes. Do these cases, in any way bear upon the question of infiltration, or lodgement of urine after the high operation?

is probably the principal source of danger in all operations of lithotomy, let us enter fully into the question of how it is to be avoided. But we must, as anatomists, be allowed first to investigate how far Mr. Pattison is entitled to the claim which he makes, of the discovery of a new fascia connected with the prostate.

The surgeons in America are neither inclined to give Mr. Pattison the merit of correctness in his anatomy, nor the credit of having made a discovery. Dr. Jameson, of Baltimore, very clearly demonstrates that Mr. Pattison has been incorrect in his anatomy, and that he has deceived himself, in consequence of the particular manner in which he conducted the dissection. But those gentlemen are not so well convinced, that there is not some discovery; for Mr. Pattison, in reply to one of his critics, says, "It was really and truly to me a discovery; it was considered as such, by all the professional friends with whom I conversed on the subject in Edinburgh, London, and Paris." As he ranks among his London and Parisian friends, Mr. Cooper and M. Dupuytren, we cannot be surprised that his opponents should have some hesitation in conceiving, that those gentlemen could have allowed it to be a discovery, if it had been before noticed. Some of his critics have brought against him the work of Mr. Colles, but this Mr. Pattison answers by saying that he acknowledges in his Essay, that he believes that anatomist had seen the same fascia, although he drew no deductions from what he saw. Another opponent brings the 5th N°. of Mr. C. Bell's Surgical Reports, in which he has given a plate of the same fascia, taken from the thesis of his pupil Dr. Gardiner. To this Mr. Pattison replies, "I have not been able to obtain a sight of this number of Mr. Bell's Reports, and can say nothing of the fascia alluded to by Dr. Gardiner, but this will in no way militate against my claim, as the work quoted was not published until eighteen months after I made public, as a discovery, the prostate fascia." Mr. Pattison is here in an error regarding the date of this plate. The drawing was made in the dissecting room of Great Windmill-street, in 1813, and it was printed in the same year by Dr. Gardiner in his probationary essay, on entering into the College of Surgeons in Edinburgh. Although Mr. Pattison was then in Glasgow, he might not have had an opportunity of seeing the thesis, but it is rather curious that he should in the investigation of this subject have omitted to examine the splendid book from which the following quotation is taken, and which was published before the Memoir of Scarpa, which Mr. Pattison informs us led him to make the discovery.

"Fig. III. Pl. 1.—This figure exhibits the fascia which comes down from the bladder and os pubis to cover the fore part of the prostate gland;" and in the next page, "further it is this fascia,

which, being imperfectly cut across in the operation for the stone, permits the urine to lodge behind it, and thus produces abscess. The abscess forming behind the fascia, its progress is directed towards the rectum."—Engravings of Specimens of Morbid Parts preserved in the Author's Collection. By Charles Bell, fol. 1813.

Mr. Pattison, after showing that though many anatomists must have seen the circulation prior to the time of Harvey, says very properly, that since they could draw no conclusions from what they saw, that they were not entitled to the merit of making the discovery; so, "if physiological inferences are necessary to constitute an anatomico-physiological discovery, certainly surgical deductions are equally required to an anatomico-chirurgical one; and I trust that even W. will allow, that I have been the first to draw these inferences from the connexions of the prostate fascia." So far Mr. Pattison is correct, in conceiving that a *discovery* will not be attended to, unless some deduction be drawn from it; but we trust he will allow, that although Mr. Bell has drawn a very opposite inference from his observations of this fascia, that Mr. Pattison will still consider it sufficient to entitle Mr. Bell to give in his claim as the discoverer of the fascia; but we believe W. (one of Mr. Pattison's opponents) was correct in saying, "in fact, these parts appear to be spoken of, both by Mr. Colles and Mr. Bell as matters of course; as things which have been long known, and to claim which as discoveries, would undoubtedly in their eyes seem ridiculous in the extreme."

Enough has now been given to entitle us to say, that the discovery of this fascia may be ranked with that of the hundred fasciæ, which were made in the groins of subjects in Scotland and elsewhere, when the anatomy of hernia was in fashion, and that it was really known to some anatomists in London, notwithstanding Mr. Pattison's assertion. "It would appear, previous to the publication of my sentiments regarding the connexion of the prostate fascia, none of the anatomists in Europe, to whom I had demonstrated it, were aware that such a fascia existed, and in America no surgeon ever thought of this connexion."

Let us now come to the practical question; and here we shall find a difference of opinion, to canvass which, is much more important than the title to the discovery. Mr. Pattison says, that infiltration of urine takes place, in consequence of cutting this fascia; and to establish this opinion, he gives us a diary of his thoughts on the operation of lithotomy. He tells us that, in his "boyish days," he imagined that patients died after the operation of lithotomy, in consequence of too small an incision. We suspect that his first impressions were more correct than his

present opinions; for he tells us, that he has come by a train of reasoning to satisfy himself, that the safe operation, is to cut only a very small part of the prostate, and to dilate the rest. This opinion is founded on the idea, that by cutting the fascia of the prostate, the urine is allowed to pass between the bladder and the rectum. "The prostate fascia separates the perineum from the pelvis, and the basis of the gland remaining uncut, it is physically impossible for one drop of urine to infiltrate into the cellular substance which connects the bladder to the rectum;" founded upon this view of the anatomy, he *proposes* an operation, in which he will not cut this fascia. Had his premises been correct, we might have agreed to his deductions, but our dissections and experiments prove, that it is physically impossible to extract a stone, of even a moderate size with the forceps, without either cutting or tearing this inelastic membrane, which Mr. Pattison calls fascia of the prostate. So the question will resolve itself into this—are the points here, to be cut or torn? for we can only take the term *dilatation* of the prostate, as meaning *tearing*. We may as well speak of dilating a piece of Stilton cheese as of dilating a prostate gland. In saying so we are aware, that we are expressing an opinion different from that of Scarpa; but our observations are founded upon experiment, and upon what is seen in the prostate of a person, from whom a stone has been extracted. But on referring to what Scarpa has said, it will be found that if he leaves any of the prostate uncut, it must be a very small portion; and by cutting according to Scarpa's plan, the fascia which is connected to the fore part of the prostate must be divided. Mr. Pattison uses the instrument of Mr. Peile of Dublin, which, he says, "possesses all the advantages which are secured in his (Scarpa's) operation, while it is freed from all the dangers attendant on the use of the gorget." Our grand objection to this instrument is, that it is used on the principle of the gorget, and with as small a staff; but the following cases will prove that it is not so free from danger as Mr. Pattison supposes. We shall not give any names, but we have reason to believe the following to be a correct report:—

"Within the last four years, four boys have been operated on for lithotomy, with Mr. Peile's instrument, by the same surgeon, at an hospital in this town. One is said to be well.

"The eldest, ———, now aged sixteen years, nine months after the operation, which was performed three years ago, was dismissed in his present situation; fæces and air passing through the urethra, incontinence of urine, the greater part of which flows per anum through a wound made during the operation in the rectum, and which is still capable of admitting two fingers into the bladder.

"———, now aged thirteen, operated on more than two years ago, was kept in the house nine months after the operation, has been since

taken in two different times, and has been there three months each time. He labours under incontinence of urine, the greater part of which flows through a fistulous opening in the perineum.

"———, aged fourteen, was operated on more than twelve months ago; since the operation, has had no power of retaining his urine."

But to return to the question of the cause of infiltration of urine. It will be evident to any who makes the experiment fairly on the dead body, that the fascia which Mr. Pattison describes, must be cut or torn in every case, where a stone the size of a walnut is extracted; so his explanation may be set aside. But the subject is one of great difficulty, when we consider the various results that occur, after operations performed apparently on the same principle. The following cases may, perhaps, throw some light on the cause of the infiltration of urine.

We were invited by a gentleman who lives about thirty miles from town, to assist him in performing an operation of lithotomy upon a hale stout man, about fifty years of age. The operation was exceedingly well performed with the gorget, and a very large stone was extracted. There was very little hemorrhage during the operation. But towards the evening a slight bleeding took place. The surgeon who had operated, was accompanied in his evening visit by a surgeon from the army, and was induced by him to put a compress on the wound, and which he fastened with a T bandage. In the morning the scrotum was found puffed up. In three days it was gangrenous, and the patient died, as one with effusion of urine, in consequence of rupture of the urethra. The cause of this effusion is so apparent, that it requires no comment, though the case furnishes a most valuable lesson.

We were the principal assistant in the operation on a gentleman, who was well known in London; his sufferings prior to the operation were excessive, and his case was rendered particularly interesting, not only on account of the manly manner in which he bore his sufferings, but also from there being a difficulty of ascertaining the real state of his bladder, in consequence of his having a stricture. There was some difficulty in introducing the staff, but the cutting part of the operation was done with great rapidity; there was very little injury done to the sides of the wound in the extraction of the stone, as it was so sandy a calculus that it was brought away in very small pieces. The only tedious part of the operation was syringing the bladder; and he was put to bed less exhausted by the operation than any patient we ever remember to have seen. There was very little blood lost during the operation. He begged to be allowed to lie upon his side; this was granted for a short time, as he expressed himself much relieved of pain by lying

in that position. The operation was performed at half past three; on coming to him about nine in the evening, we found him exceedingly well and cheerful, suffering very little pain. On looking at the wound, we were rather surprised to see no marks of blood or urine on the clothes; and we were, moreover, informed by the nurse and pupil in attendance, that no urine had passed; the patient had been lying on his back for the last three hours, having only lain a very short time on his side. Suspecting that some clot of blood might be the cause of stopping the wound, we passed the finger in, and were much astonished to find that it required considerable *boring* to introduce the finger into a wound, which six hours before admitted a large pair of forceps, enclosing a portion of stone to pass. The forefinger was passed as far as the knuckle: a small quantity of blood and urine followed. It was not deemed necessary to do more, as he was not suffering, nor had he any desire to make water; and we reasonably enough imagined that, in consequence of the irritation of the kidney, produced by the operation, that little urine had been secreted. He was then given in charge to the nurse, with an urgent request that he should lie on his back.

In the morning visit, at eight, we were told by the nurse that, towards midnight, our patient had suffered excessive pain in the bladder, of which he was relieved about six in the morning, by passing about a pint of bloody water by the penis. Through this and the two succeeding days he was remarkably well, the urine passing freely by the perineum, and he complaining only of a slight pain, quite on the pubes, for he put his hand on the hair of the pubes, when pointing to the part pained. This was imagined to be in consequence of having pulled the penis rather rudely on the staff during the operation, to prevent the escape of the urine. This pain was quite relieved by the application of leeches. He continued very well on the fourth day, but on the fifth, he was attacked by purging, with a very indistinct sort of pain in the lower part of the belly. The purging continued; he lost his spirits, and though he expressed himself as suffering no particular pain, still he said, he was sure that he should die. He died on the thirteenth day after the operation.—The cause of his death was fully explained on dissection. There was a large abscess, containing portions of gangrened cellular membrane, between the pubes and peritoneum; this abscess being exactly similar to that produced by effusion of urine in other circumstances. What was the cause of this effusion? It may, perhaps, be thought paradoxical to say, that it happened because the operation was particularly well performed; but let us try to explain the meaning of this assertion.

The operation, according to the received notions, was very well performed. The stone was extracted quickly, and without

any laceration of the parts, or any serious bleeding: but what followed? The wound was so little bruised by the extraction of the stone, that the healthy tumefaction of the injured parts became so great, that the whole extent of the wound was actually closed. In consequence of this, it required very considerable force to pass in the fore finger; this may appear to be a thing almost incredible, but we most solemnly avow that it was so. After seeing this, is it not easy to explain what took place? In the first case related, the obstruction was on the surface of the wound, and the urine was consequently driven into the cellular membrane, below the skin into the scrotum; but here the obstruction was deeper, and the urine, while it was forcibly driven into the urethra (for it came with great violence), escaped by the cut in the side of the urethra, into the cellular membrane, between the bladder and pubes, and lodging there, was the cause of the abscess.

On relating this case to a friend, he told us the following:—He had been requested to be present at an operation of lithotomy. About eight hours after the operation, he was sent for by the friends of the patient; on coming to the patient, he found him suffering such excessive pain in the bladder, that he begged that the surgeon who had operated might be sent for. The gentleman who had operated was much surprised at finding his patient so ill, and confessed he did not understand why he should suffer so much; but finding the external wound very much contracted, he most judiciously forced a catheter through it into the bladder, and thus relieved the patient by drawing off a quantity of urine. There can be little doubt, that the same consequences would have followed in this case as in the last related, had not the bladder been relieved.

If to these cases, we add the well known fact, that for the first twenty-four hours after the operation, the urine generally flows by the urethra, we shall probably come to form a correct judgment of the reason why patients die so often of effusion of urine, and knowing this, be led to the proper means of preventing it. The most obvious cause of this obstruction to the passage of the urine during the first 24 hours, is the great swelling that takes place in the tract of the wound, and it must be evident that the less violence there is done to the parts in extracting the stone, the more healthy tumefaction will there be. How is this obstruction to be guarded against? It must be obvious, that a particular position of the patient will tend to increase the obstruction, e. g. lying on the side; but the objections to this position are so apparent to every surgeon, that no one will permit his patient to lie so, except for a short time, to relieve him from the great pain immediately succeeding the operation; for it is generally found to be the easiest posture. We recollect, while assisting to carry

a gentleman into bed after the operation, that a surgeon, who operates on more patients than any other in town, checked us for not keeping the knees close, to repress the oozing of blood. He was also most particular in advising us to keep the knees of the patient together, when he was put in bed. Now it must be evident, if by this position we can prevent the blood from flowing, that we must also prevent the urine from escaping.

Is it not allowable, reasoning on these facts, to say, that the limbs may be permitted to lie apart, and thus prevent, in some degree, the chance of effusion, either of blood or urine, and at the same time afford the patient great ease. For why do we put the legs together? Is it to stop the bleeding? If so, can any one imagine that it is possible to stop a dangerous bleeding by such pressure? And if it be only an oozing, is that to be prevented? is it not rather beneficial to the patient—and that it should pass outwards; for is it not believed, that the blood being forced back into the bladder is even sufficient cause to account for death in some cases, not because of the quantity of blood lost, but from the irritation consequent upon the lodgement of the coagulum. And no one can imagine it to be of the slightest import to the ultimate closing of the wound, whether the legs be together or separate for the first 24 hours. But what is to be done, if this tumefaction is to be expected to come on, to such a degree as to close the wound, independent of the position? Can we not make a channel for the urine. Some of the older surgeons put tents into the wound; not on this principle, but rather with a view to keep to some old axioms of Hippocrates and Galen; so that, though the practice was good, it was given up, because the principle on which they did it was proved to be founded in error.

But taking a different view of the matter, would not a revival of this practice be beneficial? The tent might be withdrawn every two or three hours; but as this would teaze a patient whom we would wish to keep quiet, would it not be better to introduce a canula occasionally, or to let a canula of elastic gum remain in the wound. What will be the objections to this? That it will be a source of irritation to the bladder: but what is the irritation of a smooth canula compared to that of the rough stone which has just been removed; and such an instrument need not remain in longer than twenty-four hours, for after that period the urine generally passes by the perineum, for the great tumefaction is by that time generally much reduced. Although we know of no treatise in which the importance of this accident is dwelt upon, nor where the introduction of a canula or a catheter is laid down distinctly as a rule of practice, still by inquiry we shall find that it has been at all times more or less used. In 1556, Franco wrote upon the subject, and instead of

agreeing with Guido and others in putting sutures in the wound to close it, still he says he seldom uses tents, although they may be necessary when the stone is small, that the parts may not agglutinate; but here he remarks, "afin que l'urine sorte plus a son aise et si voudrois que la ditte tente soit perceè; auterment n'y en mettre point afin qui ne face retention du sang et de l'urine." Rosset, in arguing upon the high operation, says, "I knowingly pass by the easy retention of the tube in the penis, if the urine should happen not to pass well; which the common lithotomists are wont often to make use of, before closing the wound."

But we must be still more astonished that the necessity of introducing a canula has not been more enforced, when we see how clear Sharp is in his observations. "The first good symptoms after the operation, is the urine coming freely away, as we then know the lips of the bladder and the prostate are not much inflamed, for they often grow turgid, and shut up the orifice in such a manner, as not only to prevent the issue of the water, but even the introduction of the finger, or female catheter, so that sometimes we are forced to pass a catheter by the penis."*—p. 113.

We may now be allowed to submit, that effusion of urine is generally owing to the following causes:—

1st. That the parts at the neck of the bladder are not cut

* We are exceedingly happy to have it in our power to give the following example of the good effects of the introduction of the canula:—

December 8, 1820.—Henry Coleman, aged twenty-two, was cut for the stone in the Middlesex Hospital by Mr. Cartwright. The operation was very dexterously done, and the stone was extracted in less than three minutes after the introduction of the staff. The patient was cut at half-past twelve. At six in the evening no water had come away; a canula was then passed into the bladder. In doing this, Mr. Cartwright found the opening at the neck of the bladder very much contracted. On the canula reaching the bladder, a quantity of bloody water spurted through it, to the distance of four feet! The instrument was left in the bladder; the patient did not suffer the slightest inconvenience from it. The urine did not flow by the side of the tube, but came dribbling through it. On the succeeding day the wound appeared completely closed round the canula. On the third day the tube was removed, and *now* the opening appeared to be *larger* than on the preceding day, and the urine now dribbled freely through the wound; on the fifth and sixth days some urine passed by the penis.

Excepting a slight pain in the bowels, combined with head-ache, on the third day after the operation (but which was completely removed by a purge, though leeches were applied as a preventive of inflammation), this patient has not suffered the slightest pain since the introduction of the canula, up to this, the ninth day. Some of the urine still passes by the wound, although a very large quantity passes by the penis. The whole progress of this case entitles us to say, that it may be considered as one of the most successful on record.

We are happy to see that our opinions are strengthened by the authority of Dr. Physick, who has shown so much ingenuity in many points of surgery. He says that of late he has been in the constant habit of introducing a piece of canula into the wound, and since he began this practice his success has been much increased.

clean, that they are lacerated, and the cells of the cellular membrane consequently more opened.

2d. That in trying to push in the gorget, or from the bad management in the introduction of the forceps, a cavity is made in the cellular membrane anterior to the bladder, and thus a sac is formed for the lodgement of the urine.

3d. That the first incision is made too high; that there is not a depending opening for the urine to pass.

4th. That, although every incision may be correctly made, still the passage of the urine may be obstructed by the swelling of the sides of the wound.

It is very natural in this inquiry to make a few observations on the *high operation*.

When a student for the first time sees the dissection of the perineum, the distance at which the bladder is situated from the external parts, when he compares it with the appearance of a distended bladder when the abdomen is opened, he at once comes to the conclusion, that if a stone is to be extracted from the bladder, *here* above the pubes is the proper place to perform the operation. This was the conclusion which the older surgeons came to, and the high operation was that recommended, until experience showed it to be a very dangerous operation. Without bringing forward facts to prove this, we think it will be allowed to us, that there must have been some very strong circumstances against this apparently simple operation, which induced surgeons to give it up for an operation which to the unexperienced must appear very difficult. It is the same view of the bladder, which those who advocate the operation in our days take, forgetting that every thing is different in the living body, and particularly when a bladder has been irritated by a large stone. When such anatomists bring forward their arguments, no wonder that the practical surgeon laughs at them. There are two sets of men in our profession, anatomists, or rather dissectors who are not surgeons, and surgeons who are not anatomists. They are nearly on a par.

The high operation was given up by all the best surgeons in Europe, except in very particular cases. It has been attempted to be again introduced into this country, by a surgeon who saw the operation performed in Paris, three years ago, by M. Souberbielle. The same surgeon has since given us a book upon this operation, in which he displays great naivete in writing. Although he gives us a history of lithotomy, and in the course of the book gives us a full description of Frere Cosme's operation, and the instruments used in it, still he tells us in his introduction, "he now passed along the director (an instrument that had something of the form of a catheter), which was held by an assistant. It is not possible for any one

to conceive my astonishment; I could not comprehend for what purpose this was introduced." This honest burst, showing his ignorance of the common *sonde a dard*, may give some idea of the depth of this surgeon's information on the operation of lithotomy, prior to his visit to Paris. Though this allusion may appear invidious, still it is necessary for us to show, that if the high operation is to be recommended in *all cases*, that the recommendation should come from a surgeon who is well versed in the history of the operation.

We performed the high operation on the dead body in Paris, in 1814, when discussing the merits of the various operations of lithotomy with M. Beclard, then Chief des Travaux Anatomiques. But on returning to London, we never dreamt of describing this operation as a novelty, for we knew well, that the high operation, according to the plan of Frere Cosme, had been a regular operation in *particular* cases, in Paris, for many years. We were introduced to Mr. Souberbielle, by Sir Wm. Crichton, then at the head of the Russian staff. At that time M. Souberbielle was not reckoned, by the surgeons of Paris, a *regular man;* and this excited our curiosity to see one of the descendants of the itinerant lithotomists. We unfortunately forgot the hour at which we were engaged to see M. Souberbielle operate; but, according to our friend Crichton's report, the operation was begun with the intention of finishing it in the perineum; but on finding the stone very large, the operator said, "ma foi, c'est bien grande, il faut la faire en haut;" and then he finished the operation by cutting above. We were then led to believe, that Souberbielle performed the high operation only in particular cases.*

* A few days ago we received the following letter from M. Breschet, the Chef des Travaux Anatomiques in Paris, a gentleman whose researches upon Hernia we have already had the pleasure of laying before our readers. We are sure we cannot offend him, by making use of this hurried note, as we have already shown in former Numbers, to what a depth he has carried his investigations into some of the most interesting points of surgery:

"*Faculté de Medicine de Paris*, 18–9*bre*, 1820.

"Je me fais un vrai plasir de fournir à M. Shaw les reseignments qu'il me demande, seulement Je regrete de n'avoir que peu de chose à lui communiquer.

"Depuis l'ouvrage de Frère Cosme et celui de son neveu Basilhac aucune monographie ou traité particulier n'a eté publié en France sur la taille Hypogastrique ou haut appareil de la Lithotomie.

"Cette Methode Operatoire est employé quelquefois mais rarement par les grands practiciens de Paris et lorsqu'ils l'emploient c'est qu'ils jugent la pierre trop volumineuse pour etre extraite par la taille lateralisiè, presque toujours ils font d'abord la taille lateralisè, puis ils font la taille Hypogastrique, cependant chez les femmes ils font de suite la Lithotomie par le haut appareil.

"Il est à Paris une personne que si dit petit neveu de Frère Cosme et qui fait frequemment la taille Hypogastrique il suit en tous points le procedé de son Parent, il a meme conservé jusqu'aux instruments les moins utiles—c'est M. Souberbielle.

"Dans les derniers temps, depuis la publication des traités de Medicine ope-

It will not be denied, that before a surgeon is entitled to condemn the lateral operation, that he must be acquainted with the anatomy of the parts which are cut in this operation. We shall not hypocritically say, that we are sorry to bring forward proofs from the same author, of his having at least a most extraordinary idea of the anatomy of the perineum, for we wish, in as strong a manner as possible, to show the erroneous data upon which some imagine the lateral operation to be condemned—and in its stead, a most dangerous one proposed. It will not be necessary in proof, to quote the whole of pages 134 and 135, in which the author says—"Before we consider what is done in the lateral operation, it is necessary that we should understand the situation of the bladder and its connexions." "If, for a moment, we consider the situation of the bladder and the parts conected with it (such as the levatores ani, the obturatores interni, the coccygian muscles, the muscles from the rami of the ischium), we shall find that those muscles act on a part (i. e. the bladder) which is but slightly attached to the pubes by cellular substance and by the lateral ligaments. These I find, by repeated dissections, are seldom the same in different subjects. I say, if we consider the action of these muscles, the situation of the bladder must vary under different circumstances."

Now, we boldly ask, does any one who ever dissected these parts, either as a tyro, or in the investigation of the surgery of

ratoire de Lassus, Sabatier et du traité de Deschamps sur la Lithotomie, il y a quelques perfections introduits dans cette branche de l'art.

"1.—M. Dupuytren a publié lors de son concours pour une chaire de Medicine operatoire a la faculté de Medicine de Paris, une *Dissertation* sur la *Lithotomie*, Paris, 1812.

"2.—M. Sanson Clérc de M. Dupuytren a donné pour son Doctorat une thése sur un nouvelle methode de pratiquer la Lithotomie—Il arrive a la vessie par l'intestin rectum, Je crois avoir envoyé cetté dissertation a M. Shaw, cette methode a eté mise en pratique en France et en Italie, Je sais que dernierement Vacca Berlinghieri l'a pratiquee avec succes.

"3.—M. Dupuytren a proposé, Il y a quelques années une nouvelle methode de Lithotomie, il interesse la vessie en haut et en avant, et fait son incision exterieure sur le raphè—cette operation est decrite dans l'ouvrage qui a pour titre.—*Histoire et description de la taille laterale suivant la methode perfectionee par Cheselden, &c. traduit de l'Anglais par M. H. Guerin suivie d'une methode nouvelle pour la taille trouvee par M. Dupuytren*, 1818.

"4.—On a aussi publié des Memoires sur la Lithotomie Vaginale; M. Clemot de Rochefort et M. Flaubers de Rouen ont donné des observations.

"5. Je ne vous parle pas de l'ouvrage de votre compatriote M. Carpue, Il est, connu, jugé, et condamné par vous et par nous.

"6.—J'ai appris que votre ami M. Cross s'occupait d'une travail sur le même sujet—mieux que moi vous devez savoir ce qu'l en dit.

"Voila mon chere confrere ce que Je sais sur le point de chirurgie pour lequel vous me faites demander des renseignements, si je puis vous être agréable en tout autre point J'y suis egalement disposé.

"G. Breschet."

"P. S.—Je sais que M. Souberbielle prepare un ouvrage sur le haut appareil mais il n'a pas encore paru."

the part, imagine that the most convulsive action of the coccygeus, or obturator, which is not only separated from the bladder by a strong dense fascia, but is inserted into the femur, can have the slightest action on the bladder; or did they ever see them "as the parts connected with it?"

We shall only give one other striking instance of the anatomy:—"Hemorrhage,—a branch of the internal pudical artery, which is ramified upon the prostate, and the ramifications of which are as various as those of an oak, is frequently divided by the gorget, the bistoure-cachee, and the knife; and I have known several patients die of the division of this artery." In a foot-note he says, "a branch of this artery is sometimes wounded on withdrawing the gorget." In our dissections, which we may number at some hundreds, excepting once or twice, we never saw an artery of any importance on the prostate gland; that is, they were very numerous and small, and these we know are not the arteries to cause a dangerous bleeding. We have found, in two or three instances, the pudica communis pass along the prostate; and Dr. Barclay, who has collected every fact relating to the variety of arteries, mentions only three instances of this kind. Is it probable that the "several patients," whom the author has known to die in consequence of the division of this artery, had the misfortune to have both stone, and this very unusual distribution of the arteries? Is it not more probable that *if the patients did die of hemorrhage*, that it was in consequence of the pudica communis being cut, either in the forcing in, or in the withdrawing of the gorget? This we suppose must be the vessel the author means, when he says, "a branch of this artery is sometimes wounded on withdrawing the gorget."

The whole contents of the book have such an air of honesty, that we are perfectly satisfied the author supposed he was correct in what he wrote, and we believe that he has seen vessels of a very considerable size on the prostate, but that the vessels he saw were veins, not arteries; and he has probably been led into this mistake in consequence of using the glue injection, which all those accustomed to make preparations, know, finds a very easy passage into the veins. "In the course of my dissections and demonstrations I beg leave to observe, that I have been in the habit of injecting with glue, for a number of years, *every subject* that I have dissected."—p. 151.

We shall not here enter farther on the anatomy, we shall only remark, if this author believes that the danger of hemorrhage is from cutting the prostate, (p. 150.) what are we to think of the wonderful escape of the thousands who have had it cut?

Let us now examine how far the reasoning of the same autho-

rity would induce us to perform the high operation. We shall not argue fully upon the whole advantages, which he enumerates as to be derived from the high operation, but we shall shortly state them.

Advantage 1st. "Because it is generally performed in less time." This has not been the case in London.

2d. "There is less pain." That may be a question.

3d. "There is no fear of a fatal hemorrhage." We have already shown his fears of hemorrhage to be founded in error; we shall only add, that the instances of the pudic artery running over the fundus of the bladder, from the obturator, are more numerous, than of the artery running along the prostate.

4th. "There is no division of the prostate, nor of the inferior part of the bladder, nor is there any danger of wounding the rectum." This has been already sufficiently argued in the first part of this paper.

5th. "A stone, if of a certain size, cannot be extracted by the lateral, but may be extracted by this method." This is allowed to be *perhaps* one of the proper cases; but it is a question whether in this desperate case we ought not rather to try and break the stone, and then extract the portions by the lateral operation.

6th. "If the stone breaks, the particles can be extracted with greater ease than in the lateral operation." Does any other surgeon think so; and was not the difficulty of extracting the small portions, one of the principal reasons for abandoning the operation?

7th. "If the stone is concealed in a cyst, the cyst can be destroyed, and the stone extracted, as in Sir E. Home's case; there is also no danger of including part of the bladder with the stone, nor any danger of a fistulous opening after the operation." The latter part of this is not worth arguing, and it is a pity that an argument of so much importance should arise from a statement such as the first part of the paragraph; we shall not enter upon it fully, but put these queries: Is it generally necessary from the train of symptoms to operate where the stone is encysted? If a stone is concealed in a cyst, can it be easily felt with the staff? Will there not be at least some sensations conveyed in sounding, that should give the operator an idea that the stone is in a cyst; and were he suspicious of it, would he not say so before he commenced his operation? How many patients have died since the commencement of the practice of plunging the gorget, with what are called encysted stones, and how many examples of these cysts have been preserved and shown publicly? We shall answer this, we have seen some where the cyst was the bladder unopened. How often did Chesélden, or Rau, send their patients to bed, saying the stone was encyst-

ed; and how often has this occurred in modern times? Does not the driving the bladder before the gorget, mentioned in the early part of this Essay, account for some of the *cysts?* What can the author mean "by destroying a cyst?" Does he not know that the cyst is always on the outside of the bladder, and that if it is destroyed another wound must be made in the bladder? But neither our wishes, nor our limits, will permit us to enter farther into this subject at present.*

8th. "In case there should be any disease of the bladder, it can be examined, and proper means prescribed for the cure." We confess, we have not the slightest idea what this can mean.

9th. "In patients where the staff cannot be passed in consequence of stricture, or disease of the prostate, or where the calculus is of a certain magnitude, there is no choice of the mode of operating; either the high operation must be performed, or the patient is doomed to linger out a life of wretchedness." This truly is a desperate case; but it strikes us that the only way of being satisfied that there is a stone in the bladder, is by sounding. Would we make an opening into a man's belly, because he had the symptoms of stone? Might it not be an ulcer, or an abscess at the neck of the bladder, &c. &c.? Then if we can sound him, we may cut him by the lateral operation.

Thus far we hope we have treated the matter fairly, and if any thing is wanting to substantiate the argument with which we set out, we shall say that this author has not said one word of the danger of wounding the intestine, when the bladder is small; not one word of the danger of wounding the colon and rectum, should they adhere to the fundus of the bladder in consequence

* It would appear, that in all ages the common excuse for not extracting a stone has been, that it was enclosed in a sac; for Rosset, in a very curious Treatise on Lithotomy, printed at Paris in 1590, commences one of his chapters with this head. "It is a crafty device to say, that the stones in the bladder are covered with skins." When portions of the bladder were pulled away with the stone, he says, "for it does not seem that any of those membranes, which those impostors (with the leave of the honest and skilful operators be it spoken) put upon us, are different from the solid body and substance of those bladders that are found in the bodies of them that are newly dead." "Besides, what need would there be of cutting the bladder if the stone were invested by a membrane; for the membrane by its smoothness would prevent the stones sticking and rubbing against the bladder, and consequently would abate the pain, which nevertheless is intolerable, as is plainly evident from this alone, that it forces the unhappy persons who are thus afflicted to run, in spite of their teeth upon this hazardous way of cutting. But this one thing particularly (they themselves being judges), is a plain proof of its being a lie, by which they excuse their killing of mankind. Every stone which they extract uses, before they extract it, to be perceived by the sounding noise the catheter makes on the stone; for if that be not heard, they never attempt extracting it. But no stone invested with a membrane can be perceived by that sounding noise of the catheter against it, for the membrane intervening hinders it. Therefore no stone invested with a membrane, is ever extracted by them, whatever they may prate to the contrary."

of inflammation. The wounding of the peritoneum, he treats as a very trifle. "Though this accident is not attended with any danger, it embarrasses the surgeon."

By studying the anatomy of the pelvis, and by comparing the deductions which we draw from it, with the observations of those authorities who have written upon the subject, and by witnessing the performance of the operation on the living body, we are fully convinced that the high operation is, in the greater number of cases a most dangerous one; still, when we look to what we have written, we see that it may be supposed that some of our arguments may have been strengthened by the weakness of the modern advocate for the operation. For this reason, we are desirous of meeting the question fairly, and this we hope to have an early opportunity of doing, as we have been promised a Memoir on the subject by the venerable Scarpa. At present we shall only make a few remarks on the alleged superiority of the present mode of operating over that followed by the older surgeons.

1. It is *now* argued that the danger of the high operation consists in previously injecting the bladder, and it is, morever, said, that if we operate on a full bladder, that the case is necessarily fatal. Why this should be, we cannot imagine; but in answer to the assertion that the injection of the bladder is very dangerous, we have to remark, that Cheselden injected the bladders of the nine boys, whom he cut successfully; and that in the greater number of the cases prior to the time of Frere Cosme, this plan was followed. The objections to the injecting of the bladder are conveyed, in all the treatises on this subject, in a very vague manner; and as for the bursting of the bladder, we should suppose it very improbable that any surgeon in his sober senses would use so much force as to burst one, and particularly a bladder where the coats were thickened by the irritation of the stone.

2. That now the catheter is introduced by the urethra, which prevents the urine flowing by the upper opening. In answer to this we say, that Frere Cosme invented the cut in the perineum, because he found that though the catheter was passed, still pus and urine were lodged in the cellular membrane, above the bladder. It is a little extraordinary that it should be argued, that the catheter in the urethra should carry away all the urine, and prevent any coming from the upper opening, when it has happened, that even though a canula was in the depending part of the bladder, that still the urine has passed by the upper opening.

3. That though the bladder may be small and contracted, that it may be projected above the pubes by the sonde a-dard. Upon this M. Dupuytren, in his "Lithotomie," remarks, that

there is great difficulty in introducing a sonde a-dard, between a large stone and a thickened bladder. We have seen cases where the bladder has so closely invested the stone, that this part of the operation must have been almost impossible; for in one case the operator found that he could not introduce *a staff*, farther than into the neck of the bladder. In this case there had been for a long time incontinence of urine; and here we may remark an error into which not only the older surgeons, but a modern one has fallen, in calling a bladder thickened by the irritation of the stone a *scirrhous bladder*. Were an operation above the pubes, for the extraction of a large rough stone, where there had been great irritation, to be fatal, we have no doubt that we should hear that the cause of the patient's death was a scirrhous ulcerated bladder, and we should have it *proved* by a *sectio cadaveris*.

4. It has been said that it is easier to get at the stone by the high operation. We have measured the distance (in a stout but not a fat man), from the cut in the linea alba to the part of the bladder behind the prostate, where the stone generally lodges, and found it to be more than six inches; in the same man, the stone could be touched from the perineum by the forefinger, the distance not being four inches. Here we may add one of the arguments against the lateral operation, which is rather amusing; "but the grand and insurmountable objection to the lateral operation is, *that we are operating in the dark*." This infers that we are operating in the light, in the high operation, but *our* eyes are not quite so well adapted for looking into such a bloody pit as is made in the high operation, as to see all that is going on there, or even to examine a disease and prescribe for its cure. See 8th reason for preferring the high operation.

In conclusion we may remark, that there are not many successful cases on record, where the stone has been very large. Frere Cosme himself allows that in many cases the operation is the direct cause of peritonitis, even without epanchment d'urine dans le Peritoine—and also, that it causes abscess in the cellular tissue of the pelvis—que ces absces tiennent aux dechirures et aux desordres produits par l'operation ou a des infiltrations d'urine. This we must consider the grand objection, for with proper care we may avoid wounding the peritoneum; but if infiltration of urine, or abscess take place, we have no means of making a channel to prevent its spreading among the loose cellular membrane between the peritoneum and pubes.

We of course can have no experience in this accident, and must trust to the evidences of history, which cannot be *all* wrong. We know that two patients have been operated on in London by the high operation, and have not died; but these were young and healthy lads, where the stone was small. In

such patients, we know that the healthy inflammation will very quickly form a channel from the wound in the bladder to that in the skin; moreover, there is a fact entirely forgotten by the modern advocates of this operation, that the anatomy of the bladder in boys is very different from that of adults—that it lies much higher, and it was perhaps this which led all the old authorities to say, that this operation was almost inevitably fatal after the age of thirty.

We may sum up by saying, that all the accidents which are generally assigned as the reason why patients die after the lateral operation, are more apt to take place after the high operation; we need only particularize inflammation of the peritoneum, and infiltration of urine.

We have to apologize for the length of this article, and for the form which it has taken. We commenced it in the hope of being able to impress upon our readers, the necessity of the drawing off the urine by a canula, after the lateral operation; and in the expectation, that the facility with which the lateral operation with the knife may be performed, when a large staff is in the bladder, would be so evident, that surgeons would be convinced, that it was really an easier operation than that performed above the pubes. May we hope that we have proved that much of the danger incident to the lateral operation, may be obviated by the use of the canula. It was impossible in an Essay of this kind, at the present moment, to avoid touching upon the question of the high operation; and we trust we shall be believed when we say, that the inquiry into the merits of the proposal for the renewal of this operation has been a most unpleasant task—it has been forced upon us by the consideration of its importance, and by our desire to protest against the opinion which prevails in London, that the high operation is the one now generally recommended in the west end of the town.

[The following note was sent to the Editor with this Essay:—

"*Albany, Dec.* 10, 1820.

"Dear Sir—I think you will agree with me, that *I ought* to acknowledge myself to be the author of this paper. Yours truly,

"John Shaw."]

On the Symptoms and History of Diseases. By Dr. Marshall Hall.*

(From the Quarterly Journal of Foreign Medicine and Surgery.)

We consider Dr. Marshall Hall as a very clever and ingenious writer, though he is somewhat fastidious about his title-pages, and generally changes them in his second editions so completely, that the unwary may easily be led to purchase the same book twice. His Essay on the Mimoses is now changed to an Essay on the Disorders of the Digestive Organs; and his account of Mimosis inquieta (which Mr. John Burns and the Edinburgh journalists were old fashioned enough to consider as puerperal fever) is now changed to a Serious Morbid Affection. His able work on Diagnosis, which was found, we believe, to be rather heavy in sale, is now curtailed of its extended nosology and its tabular views, and appears under its present dapper form, of a thin octavo, on the Symptoms and History of Diseases. We think this a great improvement, and consider it so useful, that we hasten to give our readers a sketch of it, till they can procure, as all practitioners ought, copies of the original. We are anxious, indeed, to bring under the consideration of the profession a part of the science of medicine so essential to an accurate study of the whole as diagnosis has uniformly been considered. Those who are engaged in practice must feel gratified in seeing the record of those circumstances (to a superficial observer, obscure or unimportant) which enable them to establish decided indications for the treatment of diseases; and to those who are commencing the study, it cannot be too earnestly recommended that they should early try to acquire those habits of observation and discrimination which characterize the intelligent physician, and without which all other acquirements are comparatively useless in effecting the ultimate object of our studies, the relief of disease.

The subject is almost new in English medical literature, the only work of the same kind which we are aware of being Berkenhout's Symptomatology, which is, for the most part, taken from Hippocrates. On the Continent, there are several works of a similar description, among which is the German work of Dreyssis, little known here, which has been well translated into French by M. Renauldin. It is more comprehensive, but not so accurate as Dr. M. Hall's. A living author also, M. Landré-Beauvais, has published a valuable work, intitled "Séméiotique, ou Traité des Signes des Maladies." Our author seems not to have

* An Essay on the Symptoms and History of Diseases, considered chiefly in their relation to Diagnosis. By Marshall Hall, M.D. F.R.S.E. &c. London, 1822. 8vo.

made much use of these works, if, indeed, he be acquainted with them; for his book, though it manifests considerable medical learning, is far from being a compilation, as he has observed and thought for himself, and has given all his observations, even when not new, the air of originality and philosophical independence of thinking.

The sources of EVIDENCE for the identification of diseases are, the Symptoms, the History, and the Morbid Anatomy. We agree perfectly with the arguments adduced to show the necessity of the combination of the history and symptoms of diseases with the study of morbid anatomy, and on the imperfection of the knowledge derived exclusively from either source. We think this circumstance has not been sufficiently attended to, and that an undue degree of importance has been attached to the study of morbid anatomy, to the neglect of what we may call pathological physiology, or the study of the derangements of function produced by disease. We by no means wish to underrate morbid anatomy, but to prove, that, by itself, and unconnected with the observation of symptoms, it loses its utility, and may even be the means of leading us into the most serious mistakes. More than this, the study of the symptoms and history of diseases supplies a deficiency in morbid anatomy, namely, such instances of morbid affection as consist in derangement of function and leave no trace under the scalpel of the anatomist. And it is one of the objects of the history of diseases to trace the transitions, in mixed cases, of deranged function into deranged structure, the extensions of diseases of structure from one organ or part to another, and the superinductions, from accident or natural consequence, of one disease upon another. Dr. Hall does not profess to have exhausted his subject; on the contrary, he allows that he has devoted his attention more peculiarly to some particular circumstances in the history and symptoms of diseases, which he considers have not been sufficiently attended to by others.

The state of the *countenance* and the *attitude* of the patient are, with propriety, placed first in the rank of symptoms; and Dr. Hall tells us he has entered very minutely into the description of these appearances, to which, from their having been less observed by others, he has devoted more particular attention. The countenance of the patient, although a source of information too much neglected by writers on medicine, is very peculiar and highly characteristic in many diseases, and affords to the physician of experience and observation an important means of diagnosis. The kind, the stage, the changes, the mitigation and the progress of many morbid affections, are accompanied and denoted by corresponding states of the countenance. The observations made above relative to the attention claimed by the dif-

ferent conditions of the countenance, are equally applicable to the subject of the attitude and motions of the body, in general, in different diseases; for although the attitude, in certain diseases, is so remarkable, as absolutely to have challenged observation, yet, in general, this point has been too little noticed, and its indications too little explored. Hippocrates and Celsus have particularly noticed the attitude of patients.

The individual circumstances referred to the COUNTENANCE, are the changes in the viticular surface, the cutaneous circulation, the cellular substance, the muscular system, some particular features, and the general expression. In the common acute fever there is a diffused vivid flushing of the countenance, frequently with considerable turgidity; the tunica albuginea is apt to be suffused, and there is great febrile heat. There are also general anxiety, tremor of the lips in speaking, and a rapid movement of the nostrils, from hurry in respiration. In acute symptomatic fever the countenance has a very different aspect. The heat, turgidity, and flushing, the suffusion of the eyes, tremor of the lips, and hurried motion of the nostrils, are absent, the surface being frequently moist. There is also usually an appearance peculiar to the primary disease. In common slow fever, instead of tumidity and suffusion, there are shrinking, partial flushing of the cheeks only, emaciation, and frequently a pallid and sallow hue: the cheeks fall, and the bony parts appear prominent: the surface becomes warm, dry, and rough; the lips and tongue are dry and tremulous, and are not moved with the usual freedom in articulation: the teeth are frequently affected with sordes or mucus. In chronic symptomatic fever the appearances are peculiar. There is a characteristic expression of disease, which strikes even the common observer. The surface and complexion are cool and pale, or affected with transient or partial heat and flushing, usually without sallowness, frequently with slight lividity, sometimes with cool moisture: there are emaciation and shrinking; the cheeks falling in, the action of the muscles becoming apparent, and the skin forming into folds. These appearances are however, as might be expected, greatly modified by the nature and seat of the original disease. In the milder form of typhus fever the countenance is equally unattended by deep flushing and tumidity, or by shrinking; but it is highly characterized by an expression of languor, feebleness, anxiety, and indisposition, and by tremor observed in the lips, and on speaking; the eyes are frequently suffused; the cheeks slightly flushed; the surface affected with a moderate degree of warmth. In the severe forms of typhus fever, the countenance is marked by great debility and tremulousness of the muscles, with great shrinking; the bones are more prominent; the intervening spaces more sunk and de-

pressed than natural; the surface is sometimes slightly flushed, sometimes cool and clammy. The eyelids are frequently partly closed; the eyes suffused, dull, and covered with a film of mucus: the mouth is partly open; the teeth and lips covered with dark glutinous sordes: articulation is imperfect, and attended with great effort: there is tremor and inadequate action of the lips and tongue; there is often superadded, the appearance of delirium, or of coma; of congestion; of collapse or of sinking.

There are not perhaps any cases in which greater information may be derived from the countenance of the patient, than in those peculiar series of morbid affections which our author has so ably described in another place, under the "outré" name of Mimoses, and to which he fairly allows that the more usual appellation of dyspepsiæ is sufficiently applicable. There are few, however limited their opportunities, who must have failed to observe the striking manner in which the most acute diseases may be "mimicked" by affections in themselves not immediately dangerous, but easily rendered so by the mistaken employment of remedies most likely to relieve their prototypes. Such are Dr. Hall's mimoses, and the endless varieties of hysteria.

The most severe or acute form of this affection is accompanied with some paleness and sallowness, and a dark hue about the eye; the cutaneous vessels exude a little oily perspiration; the prolabia are slightly pale and livid; the muscles of the face, and especially of the chin and lips, are affected with a degree of tremor, particularly on hurry or surprise, or on speaking. Another variety of this affection constitutes the well-known chlorosis. (See *Quart. Journ. For. Med.* iv. 324.) A more chronic form is attended by a state of sallowness, a yellowish or jaundiced hue, a squallid or sordid paleness of complexion, or a ring of darkness occupying the eyelids, and extending a little, perhaps, towards the temples and cheeks, and sometimes encircling the mouth. This icterode appearance of the complexion might, in some instances, be mistaken for true jaundice, were it not, that, in the latter case, the tunica albuginea has a yellow tinge, a symptom which is absent in the former.

Under the title of the ATTITUDE are included the postures and motions of the body, the state of muscular debility, power, contraction and motion, some particular actions, and the general manner of the patient. We shall select a few instances of the manner of treating the subject. In organic diseases of the heart, in the early stages, the effects of exertion, and afterwards the particular posture, are very characteristic: in incipient or dubious cases the diagnosis is assisted by observing the effects of muscular effort, especially such as involves much change of

position and general motion: in a more advanced stage, the sufferings of the patient become more acute and permanent; a certain restlessness, anxiety, and dyspnœa, aggravated by muscular effort, take place and distinguish the case from simple hydrothorax, in which muscular motion induces far less inconvenience: in a more aggravated form the patient requires to be more raised, even to an erect posture, or one inclined forwards: at any period of the disease a sudden change of posture from the horizontal to the erect becomes necessary, from the agitation produced by a terrific dream, &c. It is scarcely necessary to allude to the contrast presented by the attitude assumed in inflammation in the abdomen with acute pain, and the restlessness and violent motion attending on spasmodic pain of the stomach, or in colic.

In the *first*, the patient lies on the back, with the thighs raised, or on the side, with the chest and thighs gently leaning towards the abdomen. The manner of the patient is soft and cautious; every muscular effort is suppressed; the arm is often put out, and the knees raised or depressed, with the utmost caution; any cough is repressed; the voice is low, the expressions short, and the patient does not complain loudly, even in acute suffering. In the *second*, the patient writhes to and fro, and constantly changes his position; or he sits in bed, bending his body forcibly forwards on the thighs; he cries out during a paroxysm of pain, and speaks in a loud and irritated tone of voice.

This difference affords one of the most striking diagnostics between the two diseases, which becomes still more satisfactory when strengthened by the coincidence of the effects of pressure on the abdomen. Dr. Hall cautions us, with great propriety, to draw our conclusions on this last point, not so much from the patient's report of his own feelings, as from the visible and less fallible effects produced on the countenance. In colic, the pain is intermittent, and during its absence the patient assumes an easy posture; on the contrary, during the remission of pain in inflammation, the same cautious posture and manner are still observed as before. We know not of any cases in which the combination of all these circumstances has afforded us more satisfactoy results than in the distinction (occasionally difficult) between puerperal peritonitis and uterine pains, recurring with short or scarcely perceptible intervals.

Retention of urine, as a symptom in acute diseases, is often denoted by a state of constant elevation of the knees, which is inexplicable until the cause is discovered.

The TONGUE forms one of the best criterions of the varieties of fever. In acute fever it is usually white, and loaded, but is seldom dry, though there is much thirst and impaired taste. In acute hectic, again, it is often natural, though sometimes whitish

or furred and frequently moist. In chronic hectic, aphthæ and soreness of the tongue, for the most part, supervene. In typhus, the tongue is sometimes white, loaded, dry, and trembles when protruded. In the severer forms, it becomes dry, parched, cracked, tender, and dark brown or black. It is then, from its soreness and tremor, protruded with great difficulty. In the cold stage of intermittent, the tongue is dry and clammy; in the hot stage, still drier; and in the sweating stage, natural, or whitish, and loaded. In scarlatina, numerous elongated, florid papillæ rise above the white covering of the tongue, which seldom occurs in rubeola. In variola, pustules sometimes appear on it. In acute dyspepsia, the tongue is, in general, loaded with a soft mucous substance, which sometimes becomes very thick, peels off, and leaves the skin red, smooth, and tender; the substance of the tongue is often swollen, œdematous, and impressed by the contiguous teeth; the red papillæ are often seen rising above the mucous load; there are indentations, often formed into creases or folds, with deep and numerous furrows; sometimes it is less loaded and indented, and its edges are red and even. In chronic dyspepsia, the tongue is more usually furred than loaded, the fur being like coarse velvet and dry; in some cases, the papillæ enlarge, in others the tongue is lobed, and in others smooth and glazed. In chlorosis, at the commencement, the tongue is pallid and tumid, with enlarged papillæ, loaded, indentatious, and sulci; it afterwards becomes cleaner, smoother, more pale, of a lilac hue, and semitransparent; the papillæ disappear, and a morbid smoothness succeeds.

The loaded and swollen tongue is usually ASSOCIATED with an oily and swarthy state of the surface and complexion; the pale, tumid, and clean tongue, as in chlorosis, is accompanied by a tendency to tumidity of the integuments and œdema of the ancles; the clean, papulated, lobulated, fissured, or morbidly smooth tongue, is united with a nearly natural state of the general surface. The morbid secretions of the tongue seem, from these facts, to be connected with a morbid secretion of the skin, —a connexion which merits farther research. The appearances of the tongue also often mark the duration of a complaint, the mere load being recent while the swollen tongue indicates a longer disease. *A practitioner who attends to these can often surprise his patients by his accurate knowledge of their case.*

In common acute fever, the GENERAL SURFACE of the body is characterized by a tumid, smooth, soft, and dry state of the surface, with a sense of glowing heat, and a florid colour; sometimes there is perspiration. In acute hectic, the surface is nearly natural; but in chronic hectic there is a chilliness, with lividity, and profuse morning sweats: while in common slow fever the skin is dry, rough, harsh, and shrinking, with acrid

heat, and sometimes the cuticle rises into miliary vesicles on the neck and breast, or it exfoliates. In typhus fever, the surface is various, and not very characteristic; the heat is nearly natural, or a little more, with dry parched skin, or clammy perspiration, and miliary or petechial eruptions. In the cold stage of intermittents, the skin shrinks, becomes pale, cold, rough, and anserine,—the temperature as low often as 74°; in the hot stage, the skin is hot and dry, the integuments tumid and injected, and the temperature sometimes 105°; in the last stage, these appearances cease, and give place to a general perspiration. In scarlatina, the integuments are very tumid and injected, temperature as high as from 105° to 112°, the rash continuous, giving a deep red hue to the skin, which is hot, smooth, soft and dry, —terminating at the decline in roughness and exfoliation. In scarlatina maligna, all these appearances are reversed, and there is even sometimes a shrinking, cold moisture, and a pale or livid rash. It is of the utmost importance to observe this, as the cold affusion, where the skin is not dry and hotter than natural, may prove instantly fatal. (*Currie's Med. Rep.* ii. 76.) In rubeola, the heat of the skin is from 99° to 102°, with little tumidity and injection, except of the face and eyelids. In dyspepsia, there is a tendency to diminished temperature, with great sensibility to cold, and perspiration excited by the slightest hurry or fatigue. In strumous disease of the mesentery, this sensibility to external cold is very great, the least draught of cold air driving the patient to hang over the fire, till the hands and legs assume a brown colour. In organic diseases of the glands of supply, such as the liver, the pancreas, the spleen, the mesenteric glands, the stomach, and the small intestines, there is considerable emaciation, and the gradual return of flesh, under a curative process, is often striking; on the contrary, in organic diseases of the glands of waste, such as the breasts, the kidneys, and the large intestines, the emaciation is less and less rapid—(See *Pemberton on the Abdominal Viscera*, ch. vi.)*

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the arm and hand;—the palm becomes cold and clammy, the nails become lilac, and their tips white and opaque. The feet and nose are also liable to become cold. In chlorosis, the hands, fingers, and nails become very pale and bloodless,—and in protracted cases the nails sink in irregularly in the middle, and break off in pieces from brittleness, so that the patient cannot take a pin out of her dress. In organic diseases of the heart, the hands become cold and livid, and the finger ends expanded. (See *Farre, Malform. of the Heart.*)

The more sudden effects of INTESTINAL LOAD and IRRITATION, so frequently mistaken for inflammation, are either or all of the following:—acute pain of the head, of the side, of the loins, of the iliac region, or of some other part of the abdomen; attacks of vertigo, of dyspnœa, of palpitation, of vomiting, of hiccup, anxiety, and distress, clammy perspiration, or, on the contrary, febrile heat. In such cases, enemata and purgatives are to be tried, but bleeding will be most injurious. These effects usually occur after great exertion, agitation, pain, fatigue, hæmorrhage, undue blood-letting or purging,—and in the course of diseases.

EXHAUSTION, from great evacuations, fatigue, or fasting, or from digitalis, produces severe rigour, long continued, succeeded by great heat, frequent pulse, and the head or the abdomen seriously affected. In slower attacks, less rigour, heat sometimes absent, throbbing pain in the head, vertigo in the erect posture, palpitation, hurried breathing, sighing, and irritability of the stomach and bowels, or constipation, and sense or fear of impending death. This, with the exception of the affections of the head, is very much like the Erethismus Mercurialis, so well described by Mr. Pearson, (*Lues Venerea*, ch. 12.) and so painfully experienced and amply detailed by the late Dr. Bateman, (*Med. Chir. Trans.* ix. 220, and M. HALL *on a serious Morb. Affect.*)

The state of SINKING is very different. It is attendant on gangrene and on death, giving the fallacious appearance of a cessation of disease. (ABERCROMBIE, *Edin. Journ.* xvi. 22.) Sometimes it is accompanied with jactitation, hiccup, and catching respiration; or with gasping, throwing off the bedclothes, sighing, feeling of the approach of death. Mr. John Hunter describes it well under the term dissolution, (*On Inflamm.* ii. 9. 3.) and Sir H. Halford has applied it to prognosis. (*Trans. Coll. Phys.* vi. 15.) These subjects our author justly thinks, are of "stupendous importance."

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sphincter muscles. It is very singular that the energies of the brain should become very similarly affected, varied only in form and degree, in the two opposite states of fulness and depletion; and the remark applies equally to the function of respiration, as affected by these opposite states of the brain, for example, in exhaustion and in plethoric apoplexy.

In the affections of RESPIRATION we must attend to the kinds of dyspnœa, of cough, of expectoration, of the effects of a full inspiration and expiration, and of the affections of the voice and articulation. By attending to these alone, for instance, to the difference of sound and continuance in cough, we may often determine the character of a disease. Dr. Hall very justly depends more upon the effects of a strong *expiration* than of a full *inspiration* in detecting inflammation of the bronchia or of the lungs, or in asthma. Why is this so much neglected by practitioners?

It is not unusual to observe, that, in varicous diseases, the frequency of the PULSE remains, when the morbid actions have apparently subsided. In such case, it is necessary to continue our attention, and watch and wait for the diminution of the frequency of the pulse; and if this event do not take place in a moderate space of time, to ascertain whether the disease be in fact subdued, or only mitigated, and pursuing its course in an insidious form. This vigilance is particularly necessary in cases of pleuritis and peritonitis.—Connected with this subject, Dr. Hall promises us, at an early period, an essay on the effects of the loss of blood, and on the use of the lancet. The subject is important, and the annals of the profession abound with valuable materials.

Under the head of the HISTORY of DISEASES we have a few rules for dispensary practice, which are satisfactory as far as they go, but by no means likely to supply the place of a system of clinical instructions,—a desideratum in English medicine.

We have said enough, and abstracted enough, to show our estimation of the tenor of the work. It does not want its faults. A main one is the absence of reference to the labours of others. The style is, perhaps, too aphoristic, and though it is in general correct and classical, we observe, more than once, the vulgarism of *named* for *mentioned*,—"I have *named* the tendency," (p. 246.) But we are at all times more willing to praise the good than to bring forward the inferior parts of the subjects submitted to our decision; and we cannot too highly praise Dr. Hall.

A Review of some of the general Principles of Physiology, with the Practical Results to which they have led. By A. P. W. PHILIP, M.D. F.R.S. Edinb.

(From the Medico-Chirurgical Review.)

Dr. Philip has laid before the public a paper with the above title, in the three last numbers of the Journal of Science edited at the Royal Institution. As it contains a retrospection of the various physiological investigations of its author, we shall take this opportunity of laying before our readers an enumeration of the physiological points ascertained by his experiments. From the nature of the paper, however, and the condensed style in which it is written, it is impossible to follow the usual plan of analysis in giving an account of it. We shall therefore lay before our readers only a statement of the facts with the general inferences to which the author has been led, and these we shall generally give in his own words. In this way we think we can best convey to the reader a distinct and correct view of the contents. Before proceeding farther, it is necessary to call the reader's attention to the sense in which Dr. Philip uses the term vital principle, the vague signification of which has led to much inaccurate reasoning. In the commencement of his paper he defines it in the following manner, and afterwards particularly requests that his definition of it may be kept in mind.

"Both the animal and vegetable world differ from inanimate matter, in affording a peculiar class of results when impressed by other agents, whether chemical or mechanical. The quality on which the peculiarity of these results depends, has been termed the vital principle. Whether this principle be something superadded to bodies, or only a peculiar arrangement of their constituent parts, we have no means of ascertaining. The fact is, that it bestows on them certain properties. It is essential that its name should convey this fact, and no more." 97.

It would appear from the experiments of Haller, that the heart is wholly independent of the nervous system; why, then, it was justly asked, is it supplied with nerves, and influenced by the passions? The first physiologists of Europe, Fontana, Procsaska, Soemmering, Bekrends, Earnest Platner, Winslow, Winkle, Johnson, Unzer, Levat, Peffinger, Scarpa, Bichat, Le Gallois, and others, have in vain endeavoured to reconcile these apparent contradictions. The committee of the Institute of France conclude their report on the experiments of Le Gallois in the following maner:—

"The foregoing, the report continues, is a short but faithful account of the principal systems by means of which authors have, since the discovery of the circulation of the blood to this day, attempted to explain the motions of the heart. On taking a general view of those in-

vented before Haller, we remark, that in all of them the nervous power is considered in one way or other as one of the conditions essential to the production of the motions of the heart, and it is always and only in the brain that they place the seat of it. The cardiac nerves therefore had a determined use in all these systems, and one could easily understand why the heart is subject to the empire of the passions: but it was impossible to explain why the circulation continues in acephalous animals, and why in experiments on animals the interruption of all communication between the brain and the heart does not stop the motion of the latter. Since the time of Haller, irritability has been the basis of all these systems. In regarding that property as essential to the fibre, and independent of the nervous influence, the circulation in acephalous animals and the different phænomena observed in the experiments alluded to, present nothing that is not easily understood; but the use of the nerves of the heart, and the influence of the passions on that organ, become inexplicable. The necessity of removing these difficulties has produced two parties among the supporters of irritability. The one, zealous favourites of the doctrine of pure irritability, called to their aid the most improbable hypothesis, and all their efforts have only served to prove how difficult it is to support the cause they espouse. The other confounded the nervous power with irritability, which they consider as one of the functions of that power; but they have been obliged to admit, either with respect to the seat or the mode of existence of the nervous power, conditions, which by their own confession are far from being demonstrated, respecting which they are not agreed, and which in the application they make of them to the motions of the heart, either do not wholly remove the old difficulties, or create new ones." 105.

M. Le Gallois imagined, that he had removed the difficulty by proving that the power of the heart is wholly independent of the brain, and derived from the spinal marrow; and the Committee of the Institute, after seeing his experiments repeated, admitted his conclusions, and published a long memoir in support of them; declaring them to be the most important which had been promulgated since the time of Haller.

Dr. Philip has pointed out the insufficiency of the grounds on which M. Le Gallois and the Committee founded their conclusions, and has demonstrated, by experiment, that the power of the heart is as independent of the spinal marrow as of the brain. He has, also, it is generally admitted, removed the difficulties in question, by showing, by direct experiment, that although the powers both of the heart and blood-vessels, are equally independent of every part of the nervous system, they are every where subjected to the influence both of the brain and spinal marrow. He has, also, demonstrated, by the 32d Experiment in his Inquiry into the Laws of the Vital Functions, that the power of the muscles resides in themselves, and consequently, that any nervous influence remaining in them after the division

of their nerves, is not the cause of their remaining excitability, as the opponents of Haller advanced in opposition to him. Dr. Philip adds,—

"The foregoing facts afford an easy solution of the difficulties stated in the report of the Committee of the Royal Academy of Sciences. The heart continues to act for some time after it is removed from the body, and performs its fuctions in the fœtal state, when no brain, and, as the committee ought to have added, no spinal marrow has existed, because it has no direct dependance on any part of the nervous system. The heart is supplied with nerves, and subject to the influence of the passions, because, although independent of this system, it is capable of being influenced through it."

By the whole of Dr. Philip's experiments on the subject, he has pointed out that the difference in the nature of the muscles of voluntary, and those of involuntary motion, is not, as Haller maintained, that the one is subjected to the command of the nervous system, while the other is placed beyond it; but that, both being subjected to its influence, this difference consists in its affording the sole stimulus of the former, but only an occasional stimulus to the latter class of muscles. Dr. Philip observes,—

"Much has been said of the cause of the one set of muscles being subjected to the will, while the other is independent of it; but if the mind be freed from preconceptions, we can surely be at no loss to account for this difference, when we know that the muscles of involuntary motion are all exposed to the constant, or constantly renewed action of stimuli over which the will has no power; while the sole stimulus of the muscles of voluntary motion is wholly subjected to it. Besides, the action of the former muscles produces no sensible effect. We will to move a limb, not to excite a muscle; we wish to handle, for example, and on trial find that we can move the fingers, but there is no act of volition which could be performed through the medium of the heart and blood-vessels. If we had no wish to handle, the muscles of the fingers, of course, could never have become subject to the will. Few have any command over those of the external ear; and it deserves to be remarked, that the will influences the rectum and bladder, the only internal organs which can assist in accomplishing an end desired. It seems unnecessary to add, after what has been said, that the ganglions by no means intercept the influence of the brain and spinal marrow in its course to the muscles of involuntary motion, as some have supposed."

Dr. Philip prosecuted his inquiries into the relation between the muscular and nervous systems, much farther than Haller attempted to go, and has, by various experiments, ascertained that the nervous influence is supplied to these different classes of muscles from different sources. To the muscles of voluntary motion, it is supplied from the several parts of the brain

and spinal marrow from which their nerves originate, while to the muscles of involuntary motion, he has proved by an extensive set of experiments, it emanates from every part of the brain and spinal marrow, through the interposition of the ganglions, which are thus shown to be the means of concentrating the influence of every part of these organs, to bestow it on all those parts which participate the influence of the ganglionic nerves. The use of the ganglions is thus pointed out, which could not, of course, have been conjectured till it was demonstrated, that the parts supplied by ganglionic nerves, share the influence of every part of the brain and spinal marrow. Dr. Philip remarks,—

"We may easily conceive why the muscles of voluntary motion are excited when those parts of the brain or spinal marrow from which they receive their nerves are stimulated; but it seems at first view more difficult to account for the heart and other muscles of involuntary motion being subject to the influence of every part of these organs. We cannot suppose that they receive nerves from every part of them. We know, indeed, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the brain and spinal marrow; others only by small parts of them. In the latter instances, we see directly proceeding from those small parts the nerves of the part influenced. In the former instance, namely, where the part is influenced by all parts of the brain and spinal marrow, we do not see nerves going directly from all parts of these organs to the part influenced, but we see this part receiving nerves from a chain of ganglions to which nerves from all parts of them are sent. It is, therefore, evident, from direct experiments, that the nerves issuing from ganglions convey to the parts to which they send nerves, the influence of all the nerves which are received by these bodies." 266.

Dr. Philip then ascertained, by means of experiment, on which alone he has founded all his inferences, that the influence of every part of the brain and spinal marrow is thus combined for the purpose of effecting the secreting, and other assimilating processes, which, he has shown, are deranged, if the influence of any considerable part either of the brain or spinal marrow be withdrawn, while, at the same time, he pointed out, that all the parts supplied with ganglionic nerves are, more or less directly subservient to these processes. Dr. Philip continues,—

"Thus, we perceive, the necessity of every part of the function which the ganglions appear to perform. A combination of the whole nervous influence is necessary to the due formation of the secreted fluids: and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life, or for the purpose of be-

ing thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey all these fluids should be subjected to the influence by which secretion is performed. This function, it is evident, requires a more regular supply of fluids than could have been obtained, had the usual action of the vessels depended on the nervous system, which is subject to continual variation; but had not this system been capable of influencing the vessels, not only no change in it could have influenced the flow of secreted fluids, but every occasional increase of the influence of the nervous system, supplied to secreting surfaces, finding no increase of fluids to act upon, would necessarily have excited disease. Thus, it is requisite that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as from direct experiment, we have just seen, it is found to be." 274.

By the experiments on this subject brought into comparison with others, he has shown that, the processes of secretion and assimilation, as well as that process by which animal temperature is supported, are the results of the action of the influence supplied by the nervous system, on the circulating fluids. He remarks,—

"Here a question of great importance in the animal economy arises. As it appears, from the experiments just referred to, that the nervous power is equally essential with the circulation of the blood, for maintaining the functions of secretion and assimilation, what are the parts they severally perform in these functions? It is evident, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other parts. The heart and vessels of circulation, we have seen, can perform their function after the influence of the nervous system is withdrawn. The function of the secreting vessels immediately ceases on the interruption of this influence. We must suppose, therefore, either that the influence of the nervous system bestows on the extreme vessels the power of separating and recombining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

"Experiments, to which I have already referred, prove that the most minute vessels which can be seen by a powerful microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after as before the slow destruction of the brain and spinal marrow. If the power of the vessels of secretion had been lost by the interruption of the influence of the nervous system, would not this have necessarily occasioned some change in the distribution and motion of the blood in the web? The conclusion from these experiments is strengthened by others. In those in which the secreting power was destroyed either by the division of the eighth pair of nerves or the destruction of part of the spinal marrow, there did not necessarily appear to be a defective supply of fluids. In the stomach they were often as copious, some-

times more copious, than usual; and the latter was almost always the case in the lungs. The fault seemed to be, that a due change on them had not been effected. We know that the vessels of circulation possess no powers but the elastic and the muscular, or what in many of its properties resembles the latter. Can we suppose, that the vessels of secretion, which are only a continuation of those of circulation, all at once assume a different nature; or is it at all consistent with our knowledge of the phenomena of chemistry to suppose, that by any influence of the powers just mentioned, or indeed any that can be supposed to belong to vessels, could be enabled to separate and recombine the elementary parts of the blood? The first of the above positions being set aside, it seems a necessary inference from the experiments referred to, that in the function of secretion, the vessels only convey the fluids to be operated upon by the influence of the nervous system." 272.

With regard to the temperature of the animal body, Dr. Philip makes the following observations,—

"It appears, from experiments above referred to, that the destruction of any considerable portion of the spinal marrow, deranges the function of secreting surfaces. Together with this effect, it was always found to lessen the temperature of the animal, more or less, according to the extent and importance of the part destroyed.* Some years previously, Mr. Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the maintenance of animal temperature is under the influence of the nervous system, and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference. The experiments related in the inquiry just referred to, seem in a striking manner to confirm the opinion of Mr. Brodie. He found that poisons impairing the vigour of the nervous system, impair the temperature. It appears from my experiments, that lessening the extent of this system, by destroying part of the spinal marrow, has the same effect.

"Thus, it follows, that the temperature of the animal body depends on the state of the nervous system; but many observations point out that it depends also on that of the powers of circulation. When the power of the heart and vessels is greatly impaired, so that the motion of the blood languishes, the temperature falls. If by exercise, or the use of stimulants, we increase the action of the heart and vessels, the temperature in the same proportion rises. When there is a natural defect in the organs of circulation, and particularly when this defect is such as prevents the blood passing through the lungs, with the freedom necessary to its healthy state, the temperature is found below the natural standard. The reader may consult a paper on this subject, by Mr. Earle, in the seventh volume of the Transactions of the Medico-Chirurgical Society, in which there are many excellent observations. As we proceed, we shall find proofs founded on direct experiments, that the temperature depends on the state of the circulation, and particularly on the passage of the blood through

* *Exper. Inquiry*, p. 161. *et seq.* second edition.

the lungs, which to detail here, would too much anticipate some of the other parts of the subject.

"Whether caloric be a substance, or as some of the first chemists of our time are inclined to believe, only a certain motion of the particles of bodies, it is of course foreign to this paper to inquire; but it appears from the foregoing observations, and will, I think, appear still more strikingly from those I shall have occasion to add, that the maintenance of animal temperature must be ranked among the results of the action of the nervous system on the blood. It is on this account that I have elsewhere said, *that if caloric be regarded as a substance*, its evolution in the animal body must be ranked with the secreting processes; the definition of secretion, I conceive, being the evolution of a *tertium quid*, in consequence of that action." 275.

Dr. Philip maintains, that nerves are only capable of conveying impressions to and from the brain or spinal marrow, and, consequently, that these organs are concerned in all cases of sympathy; there being no evidence that impressions are ever communicated from one nerve to another, independently of the intervention of one of them, "a position," he adds, "farther illustrated by the able investigations of Mr. Charles Bell, which have afforded new and important views of the distributions and uses of certain nerves."

Dr. Philip has devoted many experiments to the ascertaining the line of distinction between the nervous and sensorial systems, which have hitherto been confounded, and has shown, experimentally, that, as the muscular power can exist after the influence of the nervous system is withdrawn, this influence can exist, and is capable of all its functions, after the sensorial power is withdrawn.

It appears, from his experiments on this part of the subject, that the muscular, nervous, and sensorial powers have no direct dependence on each other; yet are all so connected in the more perfect animals as to be incapable of long surviving each other. He has pointed out that it is through the medium of respiration that the muscular and nervous depend on the sensorial power, this being the only vital function to which the sensorial power is necessary, and the first which fails in dying in consequence of the sensorial power being the first which is withdrawn. Dr. Philip remarks,—

"M. le Gallois finds a great difficulty in explaining why respiration should cease on the removal of the brain.

"'Il est donc certain que la vie de tronc n'a son principe immédiat ni dans le cerveau, ni dans aucun des viscères de la poitrine et de l'abdomen; mais il ne l'est pas moins, que tous ces viscères sont indispensables à son entretien. Or, en considérant sous quel rapport ils le sont, les faits énoncés plus haut prouvent évidemment que, quant au cerveau, les phénomènes mécaniques

de la réspiration, c'est-à-dire, les mouvemens par lesquels l'animal fait entrer l'air dans ses poumons, dépendent immédiatement de ce viscère. Ainsi c'est principalement en tant que l'entretien de la vie dépend de la réspiration qu'il dépend du cerveau; ce qui donne, lieu à une grande difficultè. Les nerfs diaphragmatiques, et tous les autres nerfs des muscles qui serve aux phénomènes mécaniques de la réspiration, prennent naissance dans la moëlle épinière, de la même manière que ceux de tous les autres muscles de tronc. Comment se fait-il donc qu'après le décapitation, les seuls mouvemens inspiratoires soient anéantis, et que les autres subsistent? C'est la à mon sens, un des grands mystères de la puissance nerveuse; mystère qui sera dévoilé tôt ou tard, et donc la découverte jettera la plus vive lumière sur la mécanisme des fonctions de cette merveilleuse puissance.'

"This difficulty appears to me to arise from M. le Gallois's having regarded respiration as a function wholly dependent on a combination of the nervous and muscular power; whereas it seems evident, I think, that the sensorial power also shares in it. The muscles of respiration are, in the strictest sense, muscles of voluntary motion; we can at pleasure interrupt, renew, accelerate, or retard their action; and if we cannot wholly prevent it, it is for the same reason that we cannot prevent the action of the muscles of the arm, when fire is applied to the fingers. The pain, occasioned by the interruption of a supply of air to the lungs, is greater than can be voluntarily borne. Respiration continues in sleep for the same reason that we turn ourselves in sleep when our posture becomes uneasy. It continues in apoplexy for the same reason that the patient generally moves his limbs if they are violently irritated. If respiration continues in apoplexy when no irritation of the limbs, however violent, excites the patient to move them, it arises from the interruption of a supply of air to the lungs, producing a greater degree of irritation than we are able to produce by other means. As the insensibility increases in apoplexy, the breathing becomes less frequent; and when the former becomes such that no means can longer excite any degree of feeling, the breathing ceases.

"By a certain sensation a desire is excited to expand the chest. This is an act of the sensorium. Till this act take place, both the nervous and muscular powers, by which its expansion is effected, are inert; it is in vain that these powers exist, if the power which calls them into action be lost. Thus the removal of the brain puts a stop to respiration." 98.

While Dr. Philip maintains that it is impossible to refer the sensorial functions to any more general principle, he has, by experiments, the accuracy of which had long been questioned, but is now universally admitted, traced the nervous functions to the agency of galvanism.

He has shown, that the whole of these functions may be performed by this mean, after the nervous influence has ceased to operate, provided the parts still retain their vitality; and

has, on the other hand, proved, that that influence can be made to leave the nerves, and pass through conductors of galvanism without being rendered incapable of its function. Thus, in both ways establishing the identity of these agents.

Lastly, he has pointed out, what is perhaps of greater importance than any other fact in estimating the nature of the animal functions, that it is a necessary inference from his various experiments, that the sensorial functions alone are the results of vital parts affecting each other by their vital properties; all the other vital functions being the results of inanimate agents acting on vital parts. To prove by experiment, indeed, that the nervous influence can perform its functions after it has been made to leave the nerve and pass through other conductors, seems alone sufficient to evince that it is an inanimate agent. Hence it is that we can, by artificial means, imitate the nervous functions, that these bear so strong an analogy to the phenomena of inanimate nature, and that we can refer them to a more general law, that is, class them with other phenomena which arise from the same power, whose effects, as Dr. Philip expresses it, are modified but not wholly changed by one of the agents in these functions being possessed of vitality; while the sensorial functions, on the other hand, bear no analogy to any other phenomena, and can be referred to no more general principle, vital parts affecting each other by their vital properties only in the animal body; for all the functions of the vegetable world, according to the principles which result from Dr. Philip's experiments, arise from the action of inanimate agents on vital parts. Dr. Philip observes—

"As the properties of the vital principle do not differ from those of inanimate matter merely in degree or by any other modification, but have nothing in common with them, it follows, that when parts endowed with this principle affect each other only by their vital properties, the result must be such as bears no analogy to any of the properties of inanimate matter; and, consequently, that in all processes which have any such analogy, one of the agents must operate by the properties of this matter. We have seen that the characteristic difference in the sensorial and nervous functions is, that the former bear no analogy, the latter a very striking one, to those properties. On the other hand, we see the organs of the nervous system impressed by external objects, those of the sensorial system only through other vital parts. The nervous system is evidently the connecting link between the sensorium and the world which surrounds us. It consists of parts endowed with the vital principle, yet capable of acting in concert with inanimate matter; receiving impressions from it, and if the position just stated be correct, capable of impressing it; for there can be no stronger analogy than that which subsists between the secreting processes effected by

the influence of the nervous system, and the chemical processes which take place in such matter; and if an inanimate agent be employed in these processes, its supply and application must be regulated by the vital powers of the nervous system. Whether this agent be a distinct being, or only a peculiar modification of the particles of bodies, is not the question; all the essential inferences are, in either case, the same. The phenomena of electric animals are here in point. We see their nervous system collecting and applying, even according to the dictates of the will, an inanimate agent." 104.

We shall finish our view of the physiological part of the paper, which we have taken great pains to render connected and satisfactory, by quoting from it the following short recapitulation.

"It appears from all that has now been laid before the reader that there are three distinct powers in the animal system which have no direct dependence on each other, for we have seen the muscular surviving both the sensorial and nervous power, and the nervous the sensorial and muscular power; and nobody has supposed that the sensorial power has any dependence on either of the others, except as far as they are necessary for the maintenance of its organs, in which respect the nervous and muscular in the more perfect animal are equally, though not so immediately, dependent on the sensorial power.

"The nervous and muscular powers are, on the one hand, the direct means of maintaining the life of the animal, and on the other, of connecting it with the external world; the former receiving impressions from that world, the latter communicating impressions to it. All the functions of both powers bear a strong analogy to the properties of the world with which they are thus associated; we therefore have reason, according to principles above stated, to believe that all these functions, as is evidently the case with many of them, are the results of inanimate agents acting on vital parts.* There is none of them, as appears from the experiments which have been referred to, which may not be excited by artificial means as long as its organs retain the vital principle; and it is a remarkable fact, that they are all capable of being excited by one agent, and that an agent universally diffused, which we know from other facts to be intimately connected with the animal economy, and which in some of its most characteristic properties the influence of the nervous system resembles. Lastly, we know that an agent of the same nature with that of which we now speak, electricity, is in

* "The nervous power, by which the impressions on the organs of sense are conveyed to the sensorium, receives those impressions from inanimate matter. Even the heart is excited by inanimate agents, for although the blood be alive, it is by its chemical properties and bulk that it excites the heart and vessels, as appears from rendering the blood more or less stimulating, and greater or less in quantity."

some animals capable of being collected and applied by the organs of the nervous system.

"When from the nervous and muscular we turn to the sensorial functions, we perceive results which have lost all analogy to those of inanimate matter. They have only an indirect effect in maintaining animal life, and are excited by no impressions but those communicated through the nervous system, and consequently are the results of living parts acting on each other. Hence it is, that they are the first functions which cease when the vital power begins to fail. In the nervous and muscular functions an inanimate agent excites the languid powers of life. In the sensorial functions, the functional power and the stimulus which excites it, being equally vital powers, fail together.

"When the nature of the sensorial functions is kept in view, we cannot be surprised that the attempts to refer them to a more general principle have proved so futile. To what other principles shall we refer the effects of the vital parts of animals on each other, when it is in animals alone that such parts ever influence each other? Even in vegetable life we find nothing analogous to the sensorial functions. All its processes bear the same analogy to the properties of inanimate matter, which we observe in the functions of the nervous and muscular symptoms of animals, and are, therefore, the results of inanimate agents acting on living matter.* Much less can we look for any analogies of this kind in inanimate nature itself. Such reveries may please as the creations of the poet, but admit not of serious discussion. We are charmed with the flights of Lucretius, but we see only the perversion of philosophy in the reasonings of Hartley." 109.

In the foregoing review we have shortly enumerated the discoveries for which we are indebted to Dr. Philip. The public is left to compare them, without partiality or prejudice, with those of former or present physiologists; and to assign to Dr. Philip the place which belongs to him. He has attempted to raise no superstructure which has not experiment for its basis; and seems to have had it in view to draw a correct line between what is and what is not known. He has pointed out the vanity of inquiring into the nature of the vital principle and sensorial power, and shown that the study of them must be confined to the observation and arrangement of their phenomena. In addition to what he has done on subjects which had long been under discussion among physiologists, he has carried his investigations into the more complicated functions of the nervous system, which were very generally supposed to be placed beyond the reach of experiment, and has given an unanswerable proof of

* "It is here worthy of remark, that many phenomena render it probable that there is a continual passage of the electric influence through plants, to which both their form and position are peculiarly adapted."

his success by exhibiting these functions performed by artificial means.

It is a valuable, and it will be admitted, an unusual feature in the physiological investigations of Dr. Philip, and a striking guarantee of their accuracy, that they have led immediately to improvements in the practice of medicine. We shall quote the whole of what is said in the present paper, relating to their practical results. The importance of these results will be a sufficient apology for the length of the quotation.

"If the foregoing inferences from the various experiments which have been referred to be correct, it is reasonable to suppose that they may be beneficially applied to the practice of medicine. The view of the different functions of the animal body, and their mutual dependence on each other afforded by those experiments, cannot, in that case, fail to be of use in explaining the nature and regulating the treatment of the deviations of these functions from the healthy state, particularly in the diseases whose symptoms are most influenced by the mutual sympathy of the vital organs.

"In a Treatise on Indigestion, I have attempted its application to an extensive class of these diseases. But I here wish chiefly to direct the reader's attention to the practical results from the experiments which relate to the influence of galvanism on the animal body.

"They led me more than six years ago to the employment of this agent in diseases, which seem to arise from a defect of nervous power, particularly habitual asthma and indigestion; and an account of its effects in those diseases was published in the *Philosophical Tansactions* of 1817. It is now admitted, I believe, by all who have witnessed them, that in the former disease, and under certain circumstances of the latter, galvanism is the most effectual means of relief which we possess.

"In its employment, we must constantly guard against the inflammatory diathesis, both because it tends to produce this diathesis, and because the diseases to which it is adapted, for reasons pointed out at length in the Treatise on Indigestion, to which I have just referred, have the same tendency. As any considerable degree of the inflammatory diathesis not only obviates the beneficial effects of galvanism, but renders it injurious, the constant superintendence of a well-informed practitioner is necessary. I need not here enter more particularly into this part of the subject, which has been done in that treatise, and in my *Experimental Inquiry into the Laws of the Vital Functions*, in which the reader will find a detail of cases cured, or relieved by galvanism. To its effects in one case of considerable importance I shall beg leave more particularly to direct the reader's attention, because it is only since I last had occasion to mention the subject publicly, that I have witnessed them. Mr. Earle some time ago asked me, if I thought galvanism a probable means of relief in dyspnœa and indigestion, arising from disease of the spinal marrow. I did not hesitate to recommend a cautious trial of it, referring Mr. Earle to what I had said of such cases in the last part of the above-

mentioned inquiry. I am happy to say the result has fully answered our expectations, as appears from the following letter which Mr. Earle did me the favour to address me.

"*George Street, August* 14, 1822.

"My dear Sir,—I have much pleasure in transmitting to you the following account of the trials made with galvanism at St. Bartholomew's Hospital. The first case is that in which you witnessed its first application.

"Elizabeth Pepperall, aged 17, of fair complexion, and light hair, was admitted into St. Bartholomew's Hospital in August 1821, in consequence of an affection of the spine, which had existed for about a year and a half. At the time of her admission, it appeared, that almost all the dorsal and lumbar vertebræ were affected. She had nearly lost all power over her lower extremities and pelvic viscera; and she complained of very severe cramps at the pit of the stomach, and acute pain in the course of the costal nerves, which was much increased by pressure on the ribs, or any attempt at a deep inspiration. Her general health was much deranged; her pulse was very rapid, with occasionally severe palpitation of the heart, and constant dyspnœa. Her digestive powers were greatly impaired, she had no appetite; and could only digest a small portion of stale bread and some milk and water. Even this meal was always followed by uneasy sensations at her stomach, and an increase of headach, from which she was hardly ever free. Her bowels were obstinately costive, and the urine was scanty, and deposited large quantities of lithate of ammonia.

"She was placed on one of my invalid beds, which enabled her to remain in a state of uninterrupted rest; and after the repeated application of leeches, issues were made on either side of the dorsal spine, and subsequently in the lumbar region. The issues were kept actively open, and the strictest attention was paid to her general health. The spine very gradually became less sensible, and the power over the pelvic viscera and lower extremities slowly returned; still, however, her stomach was incapable of digesting any other food than bread and milk and water, her head-ache remained nearly unabated, and her breathing was habitually difficult. She was in this state when you saw her, and the galvanism was first administered (December 19).

"A trough containing plates of about three inches was employed. The positive wire was applied to the nape of the neck, the negative a little below the pit of the stomach. No sensation was at first produced by twenty plates; but after the sensation was excited, she could not endure more than twelve. The first sensation she experienced caused her to take involuntarily a sudden and deep inspiration. The galvanism was applied for about a quarter of an hour, at the end of which time, her breathing became much freer than it had been for many months. Of this she repeatedly expressed herself perfectly certain, at the same time she felt considerable uneasiness at the stomach. She was slightly hysterical, in consequence of the agitation she had experienced, but her breathing was tranquil during the whole evening.

"With a view to remove the tenderness in the epigastrium, leeches were applied to the region of the stomach, and the whole plan of treatment adapted to the secondary stage of dyspepsia was resorted to. When the tenderness had somewhat abated, the galvanism was repeated with more decided relief to the breathing, and without causing much uneasiness at the stomach.

"After several applications of it, the relief she experienced in her breathing lasted for two or three days, and at length it was only necessary to repeat it occasionally. The effect of its administration was uniformly the same; a most sensible and speedy relief from a state of anxious breathing to perfect ease and repose. Its beneficial effects were not, however, confined to the respiration; the powers of her stomach greatly improved, and she was able to digest a small quantity of meat or the yolk of an egg without pain. As her stomach improved, she lost the distressing head-ach, which had so constantly attended, as at one time to lead me to apprehend the existence of disease in the brain, having met with other cases in which scrofulous affection had existed in the brain and spine at the same time. Her progress from this time was uniform, and far more rapid than it had been before; and in about two months, the catemenia, which had been suspended from the commencement of the disease, returned.

"The patient was sufficiently recovered to leave the hospital, and return to her friends at Dartmouth early in July; at which time she was able to walk with very little assistance, and without experiencing the least pain in her back. On reviewing the circumstances of this case, I have not the least hesitation in stating my decided opinion of the great benefit which was derived from the employment of galvanism, not only in affording temporary relief to the breathing, but in improving the secretions, and thus materially contributing to the ultimate recovery of the patient. I feel particularly happy that the patient was in a public hospital, and that the means were employed in the presence of many intelligent medical friends and pupils, who were all equally satisfied with myself of the essential and permanent benefit which she derived from the administration of galvanism.

"It was employed in two other similar cases in the same hospital, those of Ann Baillies, and Maria May, in which it produced similar good effects, except that in one of these, the improvement of the general health, although not less than in the other cases, did not appear to have the same beneficial effect on the disease of the spine. It was tried in another case of spine disease, which was attended with fits of spasmodic asthma. These, as I was taught to expect, from the observations you have published on this subject, it failed to relieve. It is remarkable, that in the case of Ann Baillies, in which the pulse was from 140 to 150, and very weak, the use of the galvanism always rendered it stronger, and brought it down from thirty to forty beats in the minute.

"From observing the good effects of galvanism on the secretions of the stomach, I was induced to make a trial of it in a case of deafness, accompanied with a total want of secretion of cerumen

in the right ear. Its first application produced a watery secretion, which by perseverance gradually assumed the taste and all the other characters of cerumen. The hearing was greatly improved in both ears, but how far this was to be ascribed to the restoration of the secretion is rendered doubtful, in consequence of a tumour having at the same time been removed from the tympanum of the left ear by the repeated application of caustic.

"The foregoing facts you are perfectly welcome to make any use of, should you think them deserving of notice, and I am,

"My dear Sir,

"Very sincerely, yours,

"HENRY EARLE."

"It appears from the foregoing statement, that in disease of the spinal marrow, galvanism is not only capable of performing the function of the diseased part of this organ, by which the vital actions are restored to a state of health, and the patient's sufferings greatly mitigated; but that, it also, as might *à priori* be expected, by thus improving the general health, indirectly contributes to the cure of the spinal disease. With regard to the last case mentioned by Mr. Earle, in which the secretion of cerumen was restored by galvanism, this, it is evident, from what has been said, can only happen when the fault consists in a defect of nervous influence, and not in a diseased state of the vessels.

"When we compare the foregoing report of Mr. Earle with the statements which I have already had occasion to make public, respecting the effects of galvanism, in other diseases, may we not hope that if in so few years such has been the result of the employment of this remedy on the principles above laid down, a more extensive experience will still extend the advantages derived from it. I have repeatedly seen its use more successful than any other means in obstinate general nervous debility, in which transmission through the stomach and lungs has still appeared to me the best means of applying it. In certain species of fever, and other cases attended with deficient nervous energy, we have reason to believe that it will be found a valuable remedy.

"I may close these observations by observing, that when galvanism is not used to such extent as to occasion an inflammatory tendency, I have never seen any bad effect from it, except a sense of languor, similar to the feeling of fatigue, when its employment has been too long continued. The inflammatory tendency produced by it, according to my experience, is always easily removed; is never followed by any serious consequence; and, with a little care, may almost always be prevented. I have repeatedly observed that when the cure has advanced to a certain point, its judicious employment, so far from causing the inflammatory tendency, has, by improving the state of the secreting surfaces, relieved that caused by the disease." 115.

In this article, as our readers perceive, we have been strictly analytical, convinced that by such course we would best promote

the interests of science, and the well-earned reputation of the author. In fine, we have no hesitation in expressing our opinion that the various writings, discoveries, and experiments of Dr. Wilson Philip, deservedly place him high in rank among the most illustrious physiologists, pathologists, and physicians of the age in which we live.

Fatal Consequences resulting from slight Wounds received in dissection. By A. COLLES, M. D. &c. &c. Professor of Anatomy and Surgery in the Royal College of Surgeons in Ireland. [Dublin Hospital Reports, Vol. III.]

(From the London Medical and Physical Journal.)

This paper contains the detail of three interesting cases, with reflections arising from them, which we recommend to the attention of gentlemen entering upon courses of practical anatomy.

Mr. W. Hutchinson, of rather delicate constitution, received a slight scratch on the outer side of the first phalanx of the right thumb, with the knife used in opening the body of a man who had died of cynanche laryngea. The wound did not exceed one-sixth of an inch in length, and was otherwise so slight that he scarcely noticed it at the time. The cellular membrane in the body examined contained, on the external surface of the larynx and trachea, "that amber-coloured fluid, resembling melted jelly, which is so often met with in such as have been carried off by this disease." In the evening, Mr. H. was drowsy, and retired earlier than usual in consequence. Next morning, he was affected with headach, sickness at stomach, and very acute pain in the right shoulder and axilla; which symptoms increasing, he took an emetic, and followed it up with a purgative dose. On the third day, the pain had increased very much, and was confined to the shoulder-joint, in the neighbourhood of which there was some swelling, but without any discoloration of the integuments. The wound was quite free from inflammation, a small vesicle having appeared, which was flattened, and about half filled with a milky effusion. No inflamed lymphatics could be discovered, nor any enlarged glands. He had much fever, and great dejection of spirits. The usual febrile remedies were employed, fomentations were applied to the parts, and leeches to the axilla, without effect; nor did large doses of opium alleviate the pain. In this state he continued for several days, when at length some remission of the pain took place, but unaccompanied by corresponding diminution of the fever.

In the course of a few days more, he complained of pain along the same side of the thorax, and an erysipelatous redness

was perceived, running from the axilla nearly half way to the ilium: this redness soon extended downwards to the trochanter, and forwards to the groin; the integuments having a doughy feel, and receiving the impression of the finger. The surface presented, in some places, the appearance of vesicles; but these, on more minute examination, proved to be little solid elevations. The patient's strength had now become much exhausted, and was supported only by large quantities of wine. On the 15th of December, an incision was made over the fourth and fifth ribs, in hopes of giving vent to some effusion, but without this effect resulting; nor did the operation at all affect either the local inflammation or general symptoms. The pain in the shoulder had previously ceased, and at the end of the third week that of the side likewise yielded; but at this time he was attacked with pain (which soon became very violent,) along the inner edge of the biceps muscle; the arm became swelled and inflamed, some induration extending even to the pectoralis and latissimus dorsi. Fomentations and poultices were applied, and on the 1st of January, an incision made at the inner side of the arm, where some degree of fluctuation was perceptible. This gave vent to a small quantity of matter. Another abscess formed on the side; after which he rapidly got well, although the arm did not entirely recover for some weeks.

Mr. Dease, late professor of anatomy and surgery to the Royal College of Surgeons in Ireland, demonstrated the cervical nerves and brachial plexus in a woman, about forty years of age, who had died about eight-and-forty hours before, of a chronic disease of the lungs. The subject had been dissected for him. Next morning (February 14, 1819,) he awoke early with rigors and sickness; he vomited a quantity of fish, on which he had dined the day before, and afterwards bile. At half-past eleven, Dr. Colles saw him, when he regarded the case as a severe attack of the fever at that time epidemic, attended with unusual derangement of the digestive organs. He had complained, all the morning, of violent pain in the left shoulder, for which he earnestly desired to be bled; and his request was complied with at three o'clock P. M. Twenty ounces were taken from the arm by a large orifice; the blood showed no inflammatory coat. No decided relief was afforded, even at the time, and by nine o'clock in the evening the pain and fever were as great as before. A slight degree of fulness was now discovered above the clavicle, which proved exquisitely painful on the slightest pressure.

Monday, 15th.—He had spent a wretched night, from pain in the shoulder; and, when Dr. Colles visited him, "had the entire joint covered with leeches, to the amount, perhaps, of one hundred!"

From this day he appears to have been attended by Dr. Sheridan, Dr. Brookes, Mr. Richards, and the author. A draught, (what?) purgative pills, fomentations, opiate liniments, and enemata, were ordered. The draught was rejected, but the bowels were opened by the pills.

At nine o'clock next evening, he mentioned that he felt some uneasiness about the side; and, upon examination, a colourless swelling was observed, a little behind and below the posterior border of the axilla. This naturally led to the inspection of the hand, when a slight scratch, not one-fourth of an inch in length, was discovered on the thumb, and a small vesicle had formed on its side, half filled with a white effusion. This had made so little impression, that he denied at first having received any wound whatever. The pain and tenderness on pressure above the clavicle had diminished, but continued in the side, at the part where it was swollen; and the restlessness and depression were observed to suffer an exacerbation every evening about six o'clock. He took purgatives, opiates, diaphoretics, and ammonia, at different times, and seemed to experience relief up to Friday the 19th; when, after a very restless night, with some delirium, Dr. Colles discovered a vesicle on the forearm, about two inches below the incision made in bleeding him: the orifice of the vein was "inflamed in the ordinary way." Next day, his manner was quick, and his pulse continued to get smaller. The tumefaction now existed from a little below the arm-pit to the hip; it was covered with the same little hard elevations, resembling vesicles, which were mentioned in the case of Mr. Hutchinson; a slight degree of erysipelatous redness now occupied a small portion of the centre of the swelling, and by nine in the evening had spread considerably; the swelling, at the same time, extending towards the back. Some fulness of the abdomen seems to have prevailed throughout, but the bowels were kept free.

Saturday the 20th, he was very delirious, and insisted on being moved into another bed, where he lay nearly three hours without any clothes. Next day, a poultice was applied to his side, (the inflammation continuing to extend,) and cordials exhibited. A swelling was now observed in the right fore-arm, beginning about an inch and a half below the orifice in the vein, and extending about "a hand's breadth" over the flexor muscles; but no change had taken place in the vesication already mentioned. This tumour was punctured at five o'clock P. M. and about a teaspoonful of serous fluid escaped, but without the slightest reduction of the swelling.

"*Sunday evening, nine o'clock.*—Pulse in right wrist not to be felt; heat of limbs not reduced; breathing quick and la-

boured. He passed urine at five o'clock this evening; and *at ten o'clock he died.*"

On this side of the Irish Channel, we prefer dating the report which contains an account of the death of the patient after, rather than before, the event has occurred: but this *en passant.*

The morning after his death, two or three vesicles were observed on the back, and the raised hard spots continued unaltered.

Mr. Egan dissected, on the same day (February 13), part of the subject which Mr. Dease had used for his demonstration. Next day, he was attacked with rigor. On the 16th, the metacarpal part of the thumb was inflamed, with pain extending up the fore-arm: he was hoarse, and had considerable fever. By the 28th, the symptoms had greatly increased, with severe pulmonary "affection and distress." An abscess, which had formed in the axilla, was now opened, from which purulent matter, of unusually thick consistence, was discharged; the cavity extending from the pectoralis to the latissimus dorsi. From this time he gradually recovered.

These cases are of great interest, and we have given them as distinctly as the abbreviations which our limits demand would permit us: indeed, we have dwelt upon them more than we might otherwise have done, because, although serious, and even fatal, consequences have frequently occurred from wounds received in dissection, yet we have not been able to find any description corresponding to that before us, either on reference to books, or in conversing with our anatomical friends upon the subject. The author also seems to regard the train of symptoms as new in pathological record.

Mr. Dease had been in the habit of practising dissection for twenty years, so that no length of time, nor extent of habit, can be regarded as protecting the constitution from the invasion of this disease. The subject, it will be observed, was recent, and, except a thick brown fluid contained in the pericardium, there is no evidence of any thing remarkable about it: this shows the evil to be unconnected with putrefaction. Dr. Colles alludes to this circumstance, and to the general opinion, even among medical men, that the danger arising from wounds in dissection is in direct proportion to the degree of putrefaction; but, on the contrary, according to his observation, unpleasant consequences have been so rare where the subject was far advanced in putrefaction, as to induce him to think that it rather gives protection to the anatomist. There is, in our opinion, much truth in these remarks: were serious mischief to result often from scratches received in dissecting putrid subjects, the anatomical schools in this metropolis would be deserted. For our

own part, we have dissected, and seen others dissect, bodies in every stage and degree of putrescence, with perfect impunity; but have repeatedly suffered severe pains in the hands shooting up the fore-arm from opening bodies soon after death; and we think the sooner after death such examination takes place, the greater liability there is to this occurrence. This is a remark which we have often mentioned, on these occasions, to the pupils at the Westminster General Dispensary. The observation, however, is not new, as we remember to have heard it mentioned, in his Surgical Lectures, by our learned and ingenious preceptor, Dr. THOMPSON, of Edinburgh.

In order to prevent these inconveniences, Dr. Colles recommends immediately dipping the finger into oil of turpentine. This is a point, however, on which some difference of opinion seems to exist; M. PATISSIER and M. LAENNEC having both, in recent works, advanced the opinion that washing carefully with cold water is more to be trusted than burning the parts with caustic. In order to determine a question of this kind, it is necessary to form a distinct idea of the object in view by any application made use of. This, we conceive, to be simply to prevent the poison from being absorbed, and it may be effected in two ways,—either by removing the poison altogether, or by rendering the vessels incapable of taking it up. The plan of ablution can only operate in the former manner, and, where it is complete, must obviously render the second unnecessary; but then, unfortunately, the poison is not an object of sense, and therefore we have no means of ascertaining its removal. Is it not more prudent, then, to act as if it still remained, and, after careful ablution, to destroy the wounded surface with caustic? Such is the opinion of M. CHAMBON, who has paid much attention to the subject, having himself suffered twice very severely from this unpleasant accident. Various kinds of caustic application may be used, but those in the liquid form are preferable, from pervading with facility the whole of the cut or puncture; and, perhaps, there is none better than the solution of caustic alkali, so much praised by MEDERER DE WUTHWEHR: not, however, from the vulgar idea of its neutralizing the poison, of which no evidence exists, but, as we have mentioned above, on the principle of destroying the surface to which the poison is applied, and thus rendering its absorption impossible.

*Wallace and Coyne on Vapour Baths, and Fumigations.**

(From the Quarterly Journal of Foreign Medicine and Surgery.)

The utility of medicated applications to the skin in the form of vapour, has, of late years, been very generally recognised. This is principally to be attributed to the exertions of M. Galés, at the Hospital St. Louis, in Paris, and of M. de Carro, at Vienna. In our own country, it must be allowed that we have been rather backward on this point, but the establishment of institutions for the treatment of diseases of the skin, in London and Dublin, and the publication of treatises, such as those which head our present article, seem to show that the impulse has been given, and that we may expect much novel information on the subject. Their utility, however, still remains *inter dubia.*

The object of Mr. Wallace, in his first publication, seems to have been the laudable one of diffusing among the profession in this country, a more general knowledge of what has been done on the subject abroad. His attention, which he tells us, had been early directed to cutaneous diseases, was more particularly fixed on it when he became surgeon to an institution, of which he was among the first promoters,—the Dublin Infirmary for curing diseases of the skin. With these views he has given a detail of the exertions of M. Galés, which appear to have commenced in 1812, and were principally directed towards the investigation of the causes of SCABIES, and the most successful method of treating it. His experiments led him to the preference of sulphur, applied in a state of vapour: he found it successful beyond expectation, although the apparatus for its application was, in the first instance, extremely imperfect. Under these circumstances he called to his assistance M. Darcet, an able mechanic and chemist, and with him perfected the apparatus, at present so extensively and successfully employed at the Hospital St. Louis and other places. As Mr. Wallace remarks, it is for this apparatus that the profession are particularly indebted to Messrs. M. Galés and Darcet; for the efficacy of sulphureous fumigations had been long known, and probably would have been much practised, had an instrument existed, calculated to prevent the inconveniences arising from their use by the common apparatus. The success of M. Galés's treatment, and the authenticity of his reports are fully confirmed by a commit-

* 1. Observations on sulphureous fumigations, as a remedy in rheumatism and diseases of the skin, by W. Wallace, M. R. I. A. &c. Dublin, 1820.

2. Researches respecting the medical powers of chlorine, particularly in diseases of the liver, &c. &c. &c. by W. Wallace, M. R. I. A. &c. London, 1822.

3. Observations from experience obtained in various diseases, particularly those incidental to hot climates, by the external application of the nitro-muriatic acid, &c. by Phineas Coyne. London, 1822.

tee of professors of the faculty of medicine of Paris, whose report to the minister of the interior, Mr. Wallace has published.

The sulphureous fumigations which seem at first to have been confined to the treatment of scabies, and some other chronic cutaneous diseases, were in the end extended, with the greatest possible advantage, to the cure of some of the most obstinate chronic affections, such as neuralgia, sciatica, chronic rheumatism, local palsy, and hemiplegia. But for the advantages to be derived from the use of this remedy in such cases, the thanks of the profession are more particularly owing to M. de Carro, of Vienna. The perusal in 1817, of a memoir, written by M. Galés, on sulphureous fumigation, made a lively impression upon him, and he entered on the dissemination of sulphureous fumigations with the same energy and philanthropic ardour, which distinguished him at a former period in the cause of vaccination. Mr. Wallace tells us, that, much to the honour of the profession, and no doubt much gratifying to M. de Carro, his advice and example were so generally adopted, that in a short time, he forwarded forty-eight apparatuses, which among other places were established at Presburg, Breslau, Rome, Prague, Trieste, Venice, Parma, Warsaw, Brunn, and Lemberg. In 1819, M. de Carro published his Observations, in which he communicates a history of the first two hundred cases, in the order which they occurred. We have already said that M. de Carro more fully confirmed the utility of the fumigations in rheumatic, syphilitic, and arthritic cases, than M. Galés had done. Of sixty-five persons labouring under rheumatism, sciatica, and arthritis, twenty-five were cured, thirty-two much relieved, and only eight received no benefit. Of ten inveterate syphilitic affections, where mercury had been abused, five were cured, four much relieved, and only one received no benefit.

In addition to many extracts from M. de Carro's correspondence with various physicians, and which afford ample testimony of the value of fumigations, Mr. Wallace has made some observations on the mode of its application, purposely avoiding any particulars of the result of his own experience. He says, that to enable a practitioner to give this remedy a fair trial, he must first pay the greatest attention to the selection of appropriate cases; secondly, be provided with a perfect apparatus; thirdly, judiciously attend to the medical treatment of every case that requires it, before the patient enters on the employment of fumigation, both while under its influence, and after it has terminated. He states, that it is in chronic diseases that we shall principally derive benefit from fumigations; and more particularly in such as, although produced by internal causes, have become purely local, persisting after the cause that produced them has been removed. From very extensive experience, he strongly recom-

mends this treatment in a very large proportion of cutaneous diseases, particularly in most cases of prurigo, ichthyosis, pityriasis, morbus pedicularis, several forms of porrigo, all cases of scabies, many of sycosis, lupus, and ephelis, and with other remedies in pompholyx, impetigo, ecthyma and rupia. If fumigation be ever used in cases of a more acute nature, it must be with the greatest caution.

The profession is already familiar with the idea of the employment of *nitro-muriatic acid* as a substitute for mercury in hepatic diseases, &c. from the warm manner in which it was recommended by the late Dr. Scott of Bombay. (*Med. Chir. Trans.* VIII.) In the paper published by him in the Medico-Chirurgical Transactions, which contains the fullest exposition of his opinions and practice, he appears to consider that the beneficial operation of nitro-muriatic acid is to be attributed to the chlorine evolved by the mixture, but on this point he speaks undecidedly. Mr. Wallace had long adopted this idea, from various considerations, and had supposed that the different reports of the effects of nitro-muriatic acid, might depend on the more or less perfect performance of absorption by the skin, as is the case with mercury. Hence, he concluded that he could not hope to arrive at the means of administering this remedy, so as to produce at all times its specific influence on the system, except by employing it in a form more advantageous for cutaneous absorption than that in which it is generally used, or by improving the absorbent power of the skin. His researches on sulphureous fumigations had already, as we have said, demonstrated to him the superior efficacy of medicines administered in a gaseous form; particularly if the body was at that time excited by exposure to a high temperature.

At the present day great importance is attached to the diseases of the LIVER, and they seem to have apparently increased in a corresponding degree. We cannot help thinking, however, that on this point, there is much exaggeration and misconception, and that this indiscriminate and general way of arranging, and consequently of treating diseases, is very often a cloak for ignorance or indolence in those who are unable or unwilling to undertake the task of drawing a clear diagnosis. Although we differ in some degree from Mr. Wallace as to the frequency, we perfectly coincide with him as to the importance and obscurity of hepatic diseases. This seems to be also the opinion of some of the latest and best writers; thus, says Portal, "il n'y a point de maladies, qu'on connaisse moins, et qu'on traite avec si peu d'avantage que celles du foie." (*Observations sur la nature des maladies du foie*, 1813.)

The proofs which Mr. Wallace brings forward of the efficacy of CHLORINE, are principally contained in nine cases, in which

hepatic disease was successfully treated, either as a primary affection, or as the cause of secondary disorders of various kinds. We shall make an abstract of one of these to show the manner and effects of applying the chlorine.

The patient was an unmarried female, aged 35; her liver enlarged and hard, not very tender; she had a sensation of dragging in the right side of the abdomen, an incapability of lying on the left side, and a feeling of weariness in both shoulders; she was pallid and emaciated, her bowels constipated, evacuations ash-coloured, urine variable, skin harsh and dry, feet œdematous, appetite not very bad, digestion much impaired. She had used mercury even to salivation, without any benefit. Her complaints arose from mental anxiety at the distance of a year; the first symptom was suppression of the menses. Mr. Wallace began by ordering the general application of chlorine, in the dry form for half an hour each day, at the temperature of 104° Fahr. and aqueous vapour and chlorine to be directed in a stream to the region of the liver, for fifteen minutes daily, with some occasional purgative medicine. At the end of a fortnight her mouth and throat were a little sore, the secretion of saliva was increased, and a thick papular eruption had appeared over the region of the liver. She was less despondent, the bowels more lax, and the evacuations very scantily tinged by bile; she perspired occasionally, which she had not done previously. In a month her bowels were much more regular, the stools for the most part bilious, occasionally whitish, frothy, and fœtid. The size of the liver had much diminished; she slept during the greater part of the night. There was an universal but mild crop of papulæ. The application of aqueous vapour was united with that of chlorine, in consequence of the dry harsh state of the skin, and the topical application to the hypochondrium discontinued in consequence of the irritation it had caused. It is needless to detail the farther history of the case, as the improvement from this period was gradual and uniform.

When the skin is exposed to the action of chlorine, sufficiently diluted with air or aqueous vapour, at the temperature of 110° Fahr. sensations are in a few minutes excited on different parts of the surface, resembling those produced by the bites or stings of minute insects. These sensations gradually increase in number but not in severity, and at last become, by their frequent repetition, rather troublesome. The sensibility of the skin is at the same time much increased, and remains so for some time after each operation. Another immediate effect of the above manner of application is an increase of perspiration. This, Mr. Wallace does not think altogether to be attributed to the heat in this operation. One of the most remarkable effects on the skin is an eruption of very minute papulæ, an occurrence which the

author considers as desirable, having observed the use of chlorine to be most effectual when it appears. The effects of chlorine on the mucous membranes, are shown by their increased sensibility, testified by a soreness of the mouth, fauces, and œsophagus, with increased vascularity and minute ulcerations of the throat and mouth. The frequency of the heart's action, and of respiration, is certainly much increased by exposure to chlorine, most probably from the temperature at which it is applied. Mr. Wallace asserts that chlorine has the power of tranquillizing, and at the same time exciting, the nervous system; we confess that we do not see how this evident contradiction is to be explained. He allows that, on some occasions it has appeared to increase the nervous state of patients. Such are its sensible effects, and he next proceeds to speak of what he would call its insensible effects; he supposes that this medicine modifies the tone of the organic fibre in a gradual manner. We are bound to believe that he attaches some definite idea to this expression, but he should have given an explanation of it; for as he has set it down it conveys no meaning.

Chlorine may be employed in a pure or in a diluted state, either in combination with heated air or aqueous vapour: it may be applied either generally or partially, and at various temperatures. For the administration of diluted chlorine, Mr. Wallace at first used the fumigating apparatus, constructed under his direction, at the Dublin Infirmary for diseases of the skin; he has since adopted a new instrument, which is portable, and peculiarly adapted for this and similar purposes. He does not describe it, as he was unable to procure engravings in time for the publication of this treatise. Perhaps it would have been in this case better to have delayed the publication. The quantity of chlorine necessary is regulated by the state of the patient, and of the disease. The time for which it is applied, is to be at a medium, half an hour, the temperature from 98° to 120° Fahrenheit. The gas is procured from a mixture of three parts of sulphuric acid, sp. gr. 1400 with four parts of a compound of black oxyde of manganese and muriate of soda, three parts of the latter and one of the former.

In the TREATMENT of diseases by chlorine, Mr. Wallace recommends with it the use of laxatives or purgatives. He adds, that though the advantages to be derived from chlorine, used in the manner above described, are greater than those that can be obtained from nitro-muriatic acid in any form, he has no wish to see the latter method wholly abolished. Its application is more easy in the case of children, and it may be procured when the other cannot. He is not of the opinion of those who view chlorine in the light of a substitute for mercury, although there is some similarity in the effects produced by both, particularly

as regards their action on the liver and salivary glands. He concludes by disclaiming any pretension to a complete investigation of all the medical properties of chlorine, or of bringing it forward as a specific in hepatic diseases, and by assuring us that his object is to communicate such facts as may bring a new and valuable remedy to the test of experience.

We have little to say of Mr. Coyne. His pamphlet was, as he tells us, written nearly three years since, and he now presents us with it as a kind of substitute for an intended work, by Dr. Scott. There is one thing in it, which will perhaps give a more unfavourable impression of it than it really deserves, namely, the intelligence "that it is not intended solely for the perusal of gentlemen of the faculty." It principally consists of some criticisms, not altogether free from personality, on some individual who has been bold enough to disparage the virtues of the nitro-muriatic acid bath; some observations on Mr. Guthrie's trials of it, at York Hospital, Chelsea; on Dr. Scudamore's report of it; and in a few cases where he himself found it beneficial. We must allow that he fairly proves the trials of this remedy to have hitherto been very imperfectly conducted, and in a manner which by no means justifies the conclusions drawn from them. The author professes to have no wish to make a panacea of this remedy, but there seems to be a little unintentional deception here, for the share that he attributes to the liver in the production of most diseases, and the powers assigned to chlorine in hepatic disorders comes to nearly the same thing. The proportions of the acids which he recommends, are three parts of muriatic to one of nitric, and this is a point to which attention is to be paid, as the varying reports on the efficacy of the remedy may be in some degree attributed to this cause. We cannot flatter Mr. Coyne, by the supposition that his tract is likely to promote in any great degree the interests of the remedy he advocates, and we can only regret, from the high character he assigns to Dr. Scott, that he did not live to complete his purpose.

The remedies recommended in this article will doubtless soon be extensively tried, and in the end justly appreciated. To ensure this object, attention to facts, and caution in drawing inferences from them, are all that are required, and we shall feel more than satisfied if our endeavours should attain the intended objects, the advancement of our profession and the approbation of our brethren.

The Study of Medicine. By. JOHN MASON GOOD, M. D. F.R.S. Memb. Am. Phil. Soc. and F.L.S. of Philadelphia. In four volumes, 8vo. pp. 509—935—652—723. London, 1822.

(From the Medico-Chirurgical Review.)

The construction of a work like this requires an extent of erudition, and an indefatigability of research to which few medical men of the present day can lay claim. When we contemplate the various works which have emanated from the pen of Dr. Good, we must be constrained to acknowledge that his intimate acquaintance with almost all branches of science, literature, and the arts, places him in the very first rank of our learned physicians.* To this we may add, that his zeal and industry are not inferior to his talents and erudition—so that, upon the whole, it is not once in a century that we can hope to find such a union of qualifications for so arduous an undertaking. That a work of this kind was much wanted, is proved by the favourable reception accorded to the compilation of Dr. Thomas—a work of very ordinary pretensions or merit at the best, but now disgraced by English prescriptions for the use of "country clergymen and heads of families"—in imitation of those famous "Reecéan Pandects," which were once the oracles of old women and lay practitioners, after the "primitive physic" of Westley had been gathered to its forefathers. Of a very different cast and complexion is the work before us, the object of which is, to unite the different branches of medical science, physiology, pathology, nosology, and therapeutics, into a general system, so that the whole may be contemplated under a single view, and pursued under a common study.

Without being great sticklers for nosology, we are pefectly ready to admit the immense advantage which the *study* of medicine derives from a nosological arrangement—especially when that arrangement is founded, as in the present instance, on a physiological basis, "in which the diseases of the respective functions of the animal frame are connected in classes de-

* We shall only allude to some of the principal productions of this author —viz. 1. The translation of Lucretius, with copious notes, in two volumes, quarto. 2d. The Book of Job, literally translated from the original Hebrew. 3d. Pantologia, or Universal Dictionary of Arts, Sciences, and Words, by Dr. Good and Dr. Olinthus Gregory. 4th. A Physiological System of Nosology—so well known to the profession. 5th. Self-knowledge. 6th. Sacred Idyls, translated from the original Hebrew. 7th. Sketch of the Revolution in 1688. 8th. Various tracts on Prisons and Poor Houses. 9th. The History of Medicine from the earliest periods. 10th. Essay on Medical Technology; and lastly, the voluminous work before us, containing more than *three thousand pages* of closely printed letter press!

rived from those functions, and follow each other in the order in which physiologists have usually treated them."*

We agree with our author that, in grouping diseases, not arbitrarily, but in the order of connexion with their appearance in different functions and organs, it is almost impossible to obtain an insight into the nature of any one disease belonging to such groups, without obtaining some insight into the nature of the rest, or tracing out some of the laws of morbid action which are common to the whole. This is a paramount advantage attending a nosological arrangement.† On the particular nosology of our author it is now too late in the day to offer any remarks. It has been five years before the public, and has been adopted as a text-book in various medical schools, as well as by individual writers. Like all new systems of nosology, it requires a new technology—and that is unquestionably an evil. The arrangement of Dr. Good we certainly prefer to every other, though no nosological arrangement has yet appeared, without defects. To the nomenclature too we dare not object, since it is exclusively taken from the Greek, as far as regards his classes, orders, and genera—his authorities, in general, being Celsus and Galen. When he happens to wander farther, he usually supplies himself from Ætius, Cælius, Aurelianus, Diascorides, or Aristotle.

A pretty active spirit (as Dr. Good himself observes) of PHYSIOLOGY pervades the whole work—the author prefixing to every class a summary of the most important laws and interesting discoveries of physiology that relate to, or can elucidate the subjects which constitute its scope. He has also occasionally enriched the dissertation by a glance at the more striking analogies of the animal and vegetable world at large, wherever they could add to the illustration.

In the pathological department our author trusts "that nothing is newly started for the mere sake of novelty, or controverted from a mere love of disputation." In this department we shall probably have occasion to point out some principles and facts to which Dr. Good's attention does not appear to have been sufficiently drawn.

In therapeutics "the author has allowed himself a liberal range, and has, occasionally, introduced into his materia medica substances that are highly esteemed abroad, though little va-

* Preface, p. v.

† That nosological distinctions and minuteness have sometimes been carried to excess, will, we think, be allowed by any one who glances over, for instance, Plenck's Methodical Arrangement of the Diseases of the Eye, in which he will find 119 genera, comprising nearly 600 species, or distinct diseases of a single organ!

> Hei mihi! tot mortes homini quot membra; malisque
> Tot sumus infecti, mors ut medecina putetur!

lued or even known at home, or that seem, without reason, to have fallen into temporary disrepute." Dr. Good thinks, that if the pharmacopœias of former times were occasionally loaded with medicines of trifling importance, the present system of lopping and topping is calculated to make destructive inroads on their boundaries, taking from them much that is good along with some things that might be spared. A work, our author observes, which is erected on scientific principles, should know nothing of the accidental reverses of reputation which befal many of our most powerful remedies—"and still less of the varying, and too often capricious taste of the day." He has, in fact, felt himself called upon by the general voice of the times to range with some latitude over the medicinal stores afforded by art and nature, and to discriminate the respective properties of each, rather than to limit himself to a few leading productions, or to refer to the whole under the general divisions of stimulants, sedatives, cathartics, or other classific appellations.

"Whatever may be the theory or the practice advanced in the ensuing volumes, the author will generally be found to leave nothing upon trust; but to support or illustrate his assertions by authorities which he has endeavoured to give, with some degree of copiousness, from ancient as well as modern times; so as to render the work in a certain sense a summary of the general history of medicine in most ages and countries." Pref. p. xii.

The above passage will show that any attempt at a regular *analysis* of an elementary work occupying *three thousand pages* of *analytical* matter, would be quite preposterous. Nevertheless, as we consider these volumes, eminently calculated to facilitate the study, and improve the practice of the rising generation at least, we shall take a rapid survey of the six great classes into which Dr. Good has divided his subject, by which course we shall have an opportunity of affording our readers a considerable insight into the nature and execution of the work, while we offer such candid commentaries as our experience or studies may suggest.

Class I. *Cœliaca. Diseases of the Digestive Functions.*

This is the first, and perhaps the most important class of all. It occupies nearly the whole of the first volume of the work. We think Dr. Good was right in beginning with the digestive function, because it is the only one common to all classes of animals from the alderman to the hydatid*—all having an internal cavity of some kind—and all appearing to have a propensity to store it with provision of one description or other.

* Extremes approximate in this as in other instances. Thus the hydatid is a globular membrane, i. e. *all stomach*—and what more or less is the alderman?

Preceding the actual diseases of the alimentary canal, we have a neat physiological proem, or sketch, of the organs and functions of digestion—both in man and animals. These proems, which are prefixed to all the classes, are well calculated, not only to enlarge the student's views, but to lead him to cultivate a more intimate acquaintance with comparative anatomy and physiology than he might otherwise be inclined to do. A short extract or two from this portion of the work will show Dr. Good's manner and style. After alluding to the almost infinite variety of food made use of by different nations, and different classes of society in the same nation, from turtle and venison to fish-oil and potatoes, Dr. Good sums up thus:—

"Man therefore is omnivorous. But he is not the only omnivorous animal in the world; for the great Author of Nature is perpetually showing us that though he operates by general principles, he is in every instance the lord and not the slave of his own laws. And hence among quadrupeds the swine, and among insects the ant, (and more examples might be adduced if necessary,) possess as omnivorous a power as man himself, and feed equally on the fleshy parts of animals, and on grain, and the sweet juices of plants." P. 5.

Dr. Good's extensive reading and retentive memory enable him to enliven the most common elementary details by interweaving curious, uncommon, or illustrative examples, in almost every page of the work. Thus, while showing that in some animals, as in the zoophytes for instance, the alimentary canal is imperforate, he observes that man himself is sometimes born with this conformation, the office of the natural outlet being vicariously supplied by the urethra, vagina, navel, groin, or other part.

"The most extraordinary instance of accommodation of this kind which I have ever met with in the collections of medical curiosities, is that of a girl, who, from birth, was imperforate both in the anus, and meatus urinarius; in fact in the whole division of the vulva: and who to the age of fourteen, when the account was written, had regularly discharged her urine by the breasts after the manner of milk, and her feces by a natural vomiting or rejection from the stomach."* 3.

It is known that Dr. Good's first class, cœliaca, contains two orders, enterica, and splanchnica, or diseases affecting the alimentary canal, and the collatitious viscera. Odontia, or derangement of the teeth, is the first genus in the first order, and dentition is the first species. Dr. Good introduces a short, but interesting sketch of the natural history of the teeth, before en-

* Samml Med. Wahrnehmung. Band. viii. p. 29.

tering on the disorder of teething. We can only make allusion to some points in the treatment of dentition. Dr. Good is an advocate for lancing the gums when there is much irritation, but he thinks the relief is not from the discharge of blood, "but by giving a direct opening to the tooth." In this stage, says he, "if we cannot at once cut directly down upon the tooth, the lancet had better be withheld." In this precept we cannot agree with our author. From very considerable attention to the subject we are quite satisfied that the discharge of even a few drops of blood from the tumid gum gives relief, even if we do not reach the tooth. We therefore have been long in the habit of scarifying the gums repeatedly before the evolution of the tooth, and found that the pain was next to nothing, and the benefit almost certain. We agree with our author that the idea of an indurated cicatrix being left after the use of the gum lancet is quite hypothetical. On the contrary, the recently united edges of the gum, as in all other parts far more readily give way to the process of absorption than if such division had not been effected.

The cutting of the *dentes sapientiæ* often occasions considerable pain, not only in the jaw, but in the corresponding ear, requiring a very free crucial incision through the callous gum, and sometimes a removal of the tooth.

Our author relates some curious instances of those playful attempts on the part of nature to reproduce teeth at a very late period of life, viz. at the age of 60, 80, or 100. Dr. Good attended a lady who cut several straggling teeth at the age of 74, and at the same time recovered such an acuteness of vision as to throw away her spectacles, and read with ease the smallest print.

The section on tooth-ache (odontia dolorosa) occupies nearly twenty pages, and contains a great deal of curious as well as useful information for the student.* In the section on odontia stuporis (tooth-edge) there is some ingenious physiological reasoning respecting the close reciprocity of feeling at all times maintained between the teeth and the tympanum, from a union of their respective nerves. Hence the teeth often sympathize with the ear, and are set on edge, as it is expressed, by harsh, dissonant, or stridulous sounds. Perhaps the best mode of cure is habitual exposure to the cause of the affection, whereby the too delicate feeling is gradually blunted. "The grating

* We have just heard a gentleman remark that he was rather disappointed in not finding *minute* information on a particular subject, for which he consulted these volumes. The complaint was unreasonable. For *minute* information we must consult *monographs*, or distinct treatises. In a system like this, however extensive, we can expect no more than general information, and references to other and more elaborate works on the particular subjects discussed. *Rev.*

sound produced by filing a saw was probably at one time harsh and abhorrent to the ears of the sawyer; but by being inured to it, he at length hears it with indifference."

No very clear explanation, as Dr. Good observes, has yet been given of the *tartar of the teeth.* Berzelius considers it to be at first hardened mucus, during the destruction of which phosphate of lime is deposited on the enamel of the tooth. The tartar itself, when analyzed, is found to consist of concrete or dried saliva, hardened by its own earthy materials. If suffered to accumulate too much, it loosens and destroys the gums, thus causing a fetid breath. The daily use of a tooth-brush, with any of the ordinary tooth-powders, is, in general, sufficient to keep down this tartar. If it yield not to these, some of the milder acids may be safely employed—avoiding the oxalic, sulphuric, tartaric, and succinic, for *which four acids only*, the lime of the teeth has a stronger affinity than for the phosphoric with which it is combined. From these therefore we ought sedulously to abstain. If the tartar still bid defiance to the other acids, it must be scaled off by instruments.

Passing over a great many genera and species, we shall stop, for an instant, at Genus v. sp. v. FLATULENCY, a troublesome and obstinate complaint. That air is occasionally *secreted* from the mouths of the secernent vessels was the opinion of Hunter, and is far from improbable. In most cases, however, it is merely separated from the materials introduced into the stomach, when they are in the act of fermentation from imbecility of the organ, or its consociate viscera. It is, for the most part, carbonic acid gas that is extricated at those times, and is sometimes prodigious in quantity. Nor need we wonder at this; for, by the experiments of Hales, it appears that a single apple, during fermentation, will give out more than six hundred times its bulk of air—and many of the vegetables we use are more flatulent than apples. Bitters are but too often inefficient in giving that tone to the stomach and bowels which prevents the generation of gas, and aromatics give only temporary relief by the discharge of flatus, which is quickly replaced. In several distressing cases of this kind, which have lately come under our notice, we have seen considerable benefit derived from the carbonate of iron combined with rhubarb and ginger. We generally exhibit ten grains of the iron, three of rhubarb, and three of ginger, twice or thrice a day, in any convenient vehicle, keeping the patients on plain food, and enjoining them to use but little drink.

Dr. Good, after laying before us a good deal of information on this subject, collected from various quarters, concludes with the notice of two remedies which, he says, not only afford benefit at the time, but have a tonic virtue which tends to correct the disorder radically. The first of these is the tincture of the

rose wood, or rhodium lignum of the old writers, made by macerating four ounces of the wood in a pint of spirit. "It proves a warm, balsamic, and pleasant cordial in doses of from twenty or thirty drops to a drachm." The second remedy is the etherial oil, as it is now called, or oleum vini, as it was formerly designated, and which is found in the residuum of sulphuric ether. It has a strong, penetrant, and aromatic odour, and readily dissolves in alcohol or ether.

"In the current Pharmacopœia of the London College, this anodyne is imitated in the preparation called compound spirit of ether, the only form in which the etherial oil is employed as an ingredient. For the purpose I am now speaking of, however, it should be dissolved, and in double the quantity contained in the preceding preparation, in the aromatic spirit of ether, the sweet elixir of vitriol of the old dispensatories; in which case it combines its powers with the fragrant and valuable spices of this preparation, and becomes a very grateful carminative and exhilarant." 136.

The section on dyspepsia (gen. v. sp. iii.) is copious, interesting, and instructive. This is justly represented as a hydra-headed monster—"a multiform combination of maladies of which dyspepsia is the general expression." It is a vice having its seat not always or solely in the stomach, but occasionally in one or all of the circumjacent viscera—especially the liver. The etiology of this proteiform malady is ably traced out by our author, as well as its secondary effects on the system at large—especially on the lungs, where it imitates phthisis, and has been well delineated by Dr. Wilson Philip. The causes may, Dr. Good thinks, be contemplated under two heads—local and general:—"under both which they are still further resolvable into the two opposite extremes of deficient and excessive stimulation."

The *local* remote causes are indulgence in sedative or diluting substances, or stimulating and acrid materials—excesses in eating or drinking—or rigid abstemiousness, and protracted periods of fasting. The *general* remote causes may be classed under the heads of indolent and sedentary lives—intense study, "not properly alternated with cheerful conversation"—immoderate libidinous indulgence—too great muscular exertion—and last, not least, late hours, and spirituous liquors. Our author insists much on the advantages of cheerful conversation, without which, even exercise itself will be of little avail.

"For the mind, accustomed to a certain track of intellectual labour, will otherwise relapse, even while riding or walking, into the same habitual course, be dead to the most fascinating prospects around it, and become exhausted by its own abstraction. And it is to characters of this kind, perhaps more than to any other, that the amusements

of a watering place promise ample success; where the general bustle and hilarity, and the voluntary forgetfulness of care, the novelty of new scenes, and new faces, and new family anecdotes, and the perpetual routine of engagements that fill up the time with what would otherwise be trifles and frivolities, reverse the mischievous order and monotony of the past, break the sturdy chain of habit and association, and give leisure to the worn-out sensory to refresh itself." 153.

But where dyspepsy has resulted from the fashionable follies and dissipation of a town life, a change of residence to the coast, will be a change of place, but not of life.

"A total retreat from the world, the unbroken seclusion of a remote hamlet, the sober society of a few intimate friends, simple meals, and early hours; instead of close and heated rooms, crowded and motley routs, costly feasts, and midnight madrigals, are what are especially called for in this instance, but are not always to be met with in the resort of a watering-place." 154.

The fact is, that London is not the eternal city—at least on the Thames, for she is now alternately on the banks of a river, and the borders of the ocean. Yet we doubt much whether the secluded hamlet and sober society depicted by our author would have much attraction for the fashionable hypochondriac, worn out by unnatural hours, pampering diet, stimulating drinks, and sensual excesses of all kinds. It would be like putting the habitual drunkard at once upon a milk and water diet. They would both be eaten up with the horrors of their own feelings! Nothing is more fortunate for patients under such circumstances than a smart attack of some acute disease, under which, strange as it may appear, they will better bear the transition to rigid abstemiousness than when in their ordinary conditions of health.

Our author lays down judicious rules for the guidance of the dyspeptic invalid—limiting the stomach to such food as it will most easily digest—quickening the action of the bowels by gentle aperients—and augmenting their tone by warm bitters and tonics. Magnesia appears to be a very favourite remedy of our author for the correction of acidity—while for the nidorous or fetid eructations the mineral or vegetable acids must, of course, be employed. Dr. Good thinks that charcoal has not lately been used for the purposes in question to the extent it deserves. It is a powerful corrector of putridity—and may be taken in the dose of twenty or thirty grains twice or thrice a day. For the intestinal torpor, nothing, Dr. G. thinks, stands higher than rhubarb uniting an aperient and tonic power. It is often necessary, however, to combine it with such a proportion of extract of aloes, jalap, or colocynth, as may be sufficient to keep up a regular action in the bowels. Ipecacuan, in small doses, is sometimes, but not very generally, useful. "The blue pill will

generally answer better," and it is chiefly, he observes, to be employed where we have reason to apprehend that the pylorus or liver is affected. For gastrodynia, a very common concomitant on dyspepsy, we have often seen advantage from the oxyde of bismuth, in doses of five grains thrice a day. Myrrh is strongly recommended by Dr. Good as a warm tonic for the stomach. When dyspepsy is engrafted on an hysterical or hypochondriacal diathesis, we may indulge, he thinks, in stimulants of a warmer character, as camphor, asafœtida, the alliaciæ, the spicy aromatics, and even capsicum. The quassia, gentian, and columbo, offer the best bitters, if we except the camomile, uniting a warm essential oil with the bitter. If used in infusion, the vessel should be closely covered, and the menstruum not kept longer than an hour on the flowers. But we must refer to the work itself for many pages of judicious advice respecting medicines, diet, and the employment of time.*

In the section on ileus and colica pictonum, our author has, with his usual industry, collected a great deal of scattered information. We observe, however, in this, as well as in many other instances, that Dr. Good has not brought down his pathological researches to the present moment, which he might easily have done, and certainly ought to have done. Thus he has not alluded to the able observations of Abercrombie on Ileus—nor to the valuable paper on Ileus in the French Dict. des Sciences Medicales, vol. xxiii. Of these papers, however, we gave an extended account in the second volume of our quarterly series, for April, 1820, page 613 *et seq.* In the colica pictonum, or rachialgia, as it is termed in Dr. Good's Nosology, he considers the calomel and opium plan of treatment as far superior to any other that has yet been proposed.

We observe that Dr. Good divides cholera into three species—-cholera biliosa—-flatulenta—-and spasmodica. He acknowledges that it is only in the first species that there is any evidence of an *increased* secretion of bile:—in the two last, indeed, there is, he confesses, more proof of suppression than increased secretion. We have seen numerous cases of all these species, and we confess that our observations have led us to believe that even the common cholera or bilious vomiting of this country, in autumnal seasons, is not *caused* by an increase of the biliary secretion, the latter only taking place in the course of the disorder—principally, perhaps, from the action of vomit-

* Dr. Good does not mention the dandelion in this section. We have found it very useful, combined occasionally with camomile, cascarilla, or rhubarb. It increases the biliary and urinary secretions, has an aperient effect on the bowels, and improves the digestion. We generally use the infusion—three ounces of the sliced fresh root to a pint of boiling water, with a little ginger or other spice —sometimes with cream of tartar.

ing itself. Let a person take an emetic in perfect health. He first ejects the contents of his stomach—then glairy or watery fluids—and, after straining a while, bile comes off in greater or less quantity. Whoever has experienced cholera in his own person, or narrowly watched it, *from its very commencement*, in the persons of others, will be forced to confess that the above succession of events is precisely what takes place in cholera. Nor is this assertion contravened by the descriptions of our best writers, ancient or modern. True it is, Celsus tells us that bile bursts forth both upwards and downwards—bilis supra infraque erumpit. Well; but what kind of bile?—primum *aquæ* similis, interdum alba, &c. Will any modern physician contend that the *white and watery matters* first brought off the stomach are *bile?* We fancy not. The fact is, that while Celsus derives cholera from χολη and ρεω, literal *bile flux*, Trallian considers it as χολας and ρεω, which is literally *intestinal flux*. Sydenham is generally quoted in support of the bilious theory; but the English Hippocrates does not even mention the word bile in the description of the disease. "Adsunt enim vomitus enormes, ac *pravorum humorum* cum maxima difficultate et angustia per alvum dejectio," &c. p. 175. Areteus gives a more minute description of the disease than either Sydenham or Celsus, and he makes no mention of bile at the beginning of the disease. His description is truth itself. "Supra enim per vomitum erumpunt, *quæ in ore ventriculi, et gula congesta fuerant*. Infra dejiciuntur humores in ventriculo intestinisque natantes. *In primis* quæ evomuntur aquæ similia sunt:*—quæ anus effundit, stercorea, liquida, tetrique odoris sentiuntur. Siquidem longa cruditas id malum excitavit, quo si per clysterem eluantur, *primo pituitosa*, mox biliosa feruntur." Now let us see how the above description agrees with the best modern accounts. PARR expressly says, "the matters voided are at first the remains of the food; and *afterwards* bilious fluids." In Rees's Cyclopedia the article cholera, we believe, was written by Dr. Bateman, and is a very excellent composition. It states that "the bowels are seized with griping pains, and the stools, which are at first thin and *watery*, as in common diarrhœa, are passed frequently. The stomach is seized with sickness, discharges its contents, and rejects what is swallowed. In the course of a *few hours* the matter vomited, as well as that which is discharged by stool, appears to be pure bile, and passes

* We see that Celsus evidently copies Areteus, but not correctly—for Areteus makes no mention of bile, at the commencement of the symptoms, and there can be little doubt that Areteus was the practical physician, not Celsus, who is merely a compiler, and is not generally considered as having been a professional man at all. In fact, Celsus has reversed the description of Areteus, and placed the eruption of bile as the *first* symptom, whereas the Greek author makes it the *last*.

off both ways in considerable quantities." Now let any unprejudiced person contemplate these descriptions, and say whether there appears any foundation for the theory which attributes the vomiting and purging, in other words, the disease, to the presence of bile. It is now acknowledged, though the statement was long laughed at, that in the worst forms—that is, in two species out of three, superabundance of bile is quite out of the question—on the contrary, that the biliary secretion seems lessened and often entirely suppressed. Yet such is the tenacity with which we cling to preconceived doctrines, that because a discharge of bile takes place in the course of one species, (the mildest of all,) we still continue to rank the inordinate secretion of that fluid as the proximate cause of the three, in our systematic works! To us it appears that various causes, many of which we are quite ignorant of, produce an orgasm or violent irritation in the primæ viæ, the natural consequence of which is a discharge, upwards and downwards, of whatever the primæ viæ contain at the time—and that this orgasm continuing, an inordinate secretion of bile takes place, as well as an inordinate gastric and intestinal secretion, which inordinate secretion has nothing to do with the etiology of the orgasm itself—on the contrary, we believe it to be a salutary or remedial effort of Nature to remove the disease or its cause. We grant that, at the times when cholera is most prevalent, there is generally an irritable state of the biliary as well as of the gastric system, and consequently that they are both disposed to fall into violent and irregular action. We cannot regard the three species of cholera, as divided by Dr. Good, in any other light than as three grades of the same disease, differing only in violence and in danger—or rather, we should be disposed to say, that the three species depend on the degree of force or concentration in the exciting cause, whatever that may be, aerial or terrestrial, or both combined, which we believe is the case.

Of the epidemic cholera of India, Dr. Good has given as full an account as the documents before the public, and the liberality of Sir James M'Grigor in giving Dr. G. access to the army returns, would permit. We do not see that any new light whatever has been thrown on the etiology of the disease during the prevalence of the epidemic, nor has any important novelty or improvement been devised in the treatment.

In the genus HELMINTHIA, or *invermination*, as our author emphatically terms it in his own tongue, Dr. Good has, as usual, accumulated a great deal of curious information scattered through hundreds of tomes and ephemeral journals. He combats the doctrine of equivocal generation, so warmly espoused by his favourite ancient philosopher Lucretius. We now, he observes, know that an incipient stage of putrefaction, or a very short qui-

escence and exposure of animal fluids to a warm atmosphere, is sufficient to load them with animalculæ of some kind or other, "not, indeed, by fortuitously converting the constituent and decomposing principles of such fluids into the simple forms of microscopic life, but rather by affording to some few of the myriads of invisible ovula with which the atmosphere swarms, and which it may convey to them, the proper nidus, or the quickening stimulus they stand in need of."

"That the atmosphere is freighted with myriads of insect-eggs that elude our senses; and that such eggs, when they meet with a proper bed, are hatched in a few hours, into a perfect form, is clear to any one who has attended to the rapid and wonderful effects of what, in common language, is called a blight upon plantations and gardens. I have seen, as probably many who may read this work have also, a hop-ground completely overrun and desolated by the aphis *humuli*, or hop-green-louse, within twelve hours after a honey-dew (which is a peculiar haze or mist loaded with a poisonous miasm) has slowly swept through the plantation, and stimulated the leaves of the hop to the morbid secretion of a saccharine and viscid juice, which, while it destroys the young shoots by exhaustion, renders them a favourite resort for this insect, and a cherishing nidus for the myriads of little dots that are its eggs. The latter are hatched within eight-and-forty hours after their deposit, and succeeded by hosts of other eggs of the same kind; or, if the blight take place in an early part of the autumn, by hosts of the young insects produced viviparously; for, in different seasons of the year, the aphis breeds both ways." 293.

The two grand indications of treatment are first to expel or destroy the worms, and secondly, to strengthen the alimentary canal in order to prevent their reproduction. These two indications may sometimes be pursued simultaneously.

"By having the bowels loose, we prevent the accumulation of the slime in which the worms burrow; and if we have reason to believe that such accumulation has taken place, the best plan is to give active purges, as calomel, jalap, scammony, gamboge, or an intermixture of these, for its removal; and having thus, as far as we are able, exposed the naked bodies of the worms to the action of anthelmintics, we should proceed with the latter without loss of time." 313.

The list of anthelmintics is long enough to convince us that no one of them is possessed of any very strong specific powers. Dr. Good has reviewed the principal of them, dividing them into two classes—those which dislodge or destroy by a mechanical action—and those by a narcotic or other internal means. Among the former we may reckon drastic purgatives, including oil of turpentine, tin-filings, crude quicksilver, mild lunar caustic, cowhage, &c. In the latter, we may place the male fern, hellebore, tansy, savine, rue, santonicum, tobacco, &c. We think Dr. Good has dealt rather freely with the rectified oil of turpentine. "A child of ten or eleven years old may take an

ounce without any evil effect in ordinary cases." We have lately exhibited this medicine to a series of young folks, from three to fourteen years of age, in pretty active doses; and though the ultimate effects were excellent, we had reason to be several times alarmed at the primary operation of the oil—not so much from the catharsis as from the intoxicating power of the remedy.

Dr. Good thinks that there are some medicines which may be regarded as specific vermifuges—that is, as acting on the worms by some simple quality which proves highly poisonous to them without affecting the bowels. The chief of these he considers to be the bark and shoots of the cabbage-tree (areca oleracea) and the male fern. The former has been used in infusion, syrup, or powder. It only kills the worms, and consequently it is necessary to exhibit purgatives afterwards to bring them away. Of the male fern Cullen entertained no great opinion, though it is still extolled in Germany.

The section on "Proctica Spasmodica," or "Spasmodic Stricture of the Rectum," is defective in recent information—a defect which we fear we shall often have occasion to regret in the course of our survey of these volumes. In the 5th volume of the Medical Transactions of the College, Dr. Baillie has given a description of this disease, which is quoted freely by Dr. Good, who relates an interesting case that fell under his own observation, and which has now continued six years, without any material benefit from medicine. Our readers are aware that we have given in our last number a full account of M. Boyer's able paper on this subject, published more than four years ago in the Journal Complementaire, and noticed in most of the English journals of that time. It is a great pity Dr. Good did not take that paper for his text, as not only affording a more correct view of the disease pathologically, but as pointing out an operation which almost invariably cures the complaint—we mean a division of the sphincter of the gut. We have seen but one exquisitely marked case of the disease, in a medical gentleman, 42 years of age, of great mental and corporeal activity, but long subject to hæmorrhoids. It precisely answered the description by Drs. Baillie, Boyer, and Good; and after harassing the patient for more than two years, it has nearly given way, at least it now gives no pain, by a constant and unremitting use of tepid water enemata before every motion. This is a far better plan than giving laxatives by the mouth, and completely secures the irritable stricture of the sphincter, from the annoyance of the passage of hardened fæces—a source of irritation which inevitably keeps up the original disease. On this subject, however, we have said enough in our last number.

On the "Proctica Callosa," or callous stricture of the rec-

tum, Dr. Good has mustered as much respectable information as could be compressed in the short space of six pages, which is all that is dedicated to the complaint. We do not see any thing either novel or objectionable in this section.

The fifth species of genus xii. is "Proctica Marisca," the author's designation of hæmorrhoids. It is divided into four varieties, the cæca, mucosa, cruenta, and caruncularis. Rather better than ten pages are devoted to the subject of piles. We think Dr. Good has not investigated this disease in proportion to its importance—especially in a constitutional point of view, of which indeed he takes very little notice, excepting as it is connected with a gorged state of the liver. We are sorry that Dr. Good did not consult the very excellent monograph of Dr. Montegre of Paris, inserted in the 20th volume of the *Dict. des Sciences Medicales.* This work was reviewed by Sir Charles Morgan, in the 53d number of the Ed. Journal, and fully analysed by us in the 2d number of our Quarterly Series for October, 1818.

The treatment of hæmorrhoids divides itself naturally into local and general means. The general means are such as correct the morbid state of the constitution, and especially the digestive function, which is very commonly in fault. General alteratives—the blue pill, sulphur and supertartrite of potash, and electuary of senna, are the best medicines, with or without taraxacum or sarsaparilla. But a great deal of attention should be paid to the state of the constitution generally, in order to discover whether there be erratic gout, or other constitutional diathesis; or whether the hæmorrhoidal functionary movement be vicarious of some other disease. These things should be well weighed before we attempt to repel the anal determination by astringents, as nutgalls, &c. or by the local application of cold, whether in the form of glysters or externally. In this complaint, as in spasmodic constriction of the sphincter, no local means can compare with the employment of lavements, so as to secure the parts from irritation in the passage of the fæces. This process alone would go far to cure the most obstinate cases. Where there is nothing in the constitution to contra-indicate the measure, we have always found very quick and effectual relief from the following ointment, spread thick on a piece of lint, and kept on the painful and protruded piles by means of a handkerchief or T bandage.

℞. Cerat. superacet. plumb. ℥j.
Pulv. gallarum . . . ℥j.
—— opii . . . . . ℈j m. ft. unguentum.

In several instances we have found very good effects from the local application of port wine to the hæmorrhoidal tumours,

especially where there is some degree of procidentia ani. An important measure in mitigating the pains of hæmorrhoids is pressure. No pile, in fact, ought to be allowed to protrude, and thus become strangulated. On the very first appearance of an hæmorrhoidal tumour it should be pressed back within the sphincter, and prevented from protruding by a very tight bandage and pad. By this application we have enabled men to walk about with ease, in a few minutes, who were before unable to get off the sofa. When the hæmorrhoidal attack is over, and not before, bougies may be employed.

The section on the proctica exana, or procidentia ani, is replete with information. Dr. Good, however, has not apparently availed himself of an able paper on relaxed rectum by Mr. Chevalier, in one of the late volumes of the Medico-Chirurgical Transactions.*

The second order of the present class is entitled "SLANCHNICA," or diseases affecting the collatitious viscera. It comprises four genera,—Icterus,—Melæna,—Chololithus, (gall-stones)—Parabysma (visceral turgescence.)

The inquiry respecting the derivation of various names given to icterus by the ancients, is not worth our notice; and the question as to the real or exact use of the bile, will not, we fear, be settled for a few years to come. We do not see, however, how this question can be very intimately connected with the pathology and treatment of icterus.

The immediate cause of jaundice, all must allow to be, an obstruction to the flow of bile, from the liver to the intestines—its retrograde passage into the blood, whether by absorption or regurgitation, being merely an epiphenomenon which has given name to the disease. The most simple cause of obstruction, though we apprehend it is a very frequent one, is viscidity of the bile itself clogging the ducts through which it passes. This form generally comes on more slowly and insidiously than other forms, with the usual symptoms of languor, dyspepsy, yellow urine, torpid bowels, and clay-coloured stools. The eyes and then the skin assume the yellow tinge, there being little or no pain in the right hypochondrium, and rather a nausea than actual sickness of stomach. Vomiting, by antimonial emetics, in an early stage of the disease, is often of essential service in emulging the biliary ducts, and clearing away obstructions to the free discharge of the bile. Our author relates some cases, in detail, illustrative of this species of icterus, which are interesting enough in themselves, but we think they are rather misplaced in an elementary work, which should very rarely occupy space with the circumlocutory details of individual cases, unless

* See p. 465 of the 1st volume of the *Analytical* Series of this Review.

of a very rare or extraordinary nature. We may just remark here that Dr. Good occasionally commits what we might call pleonisms of expression, and which we did not expect in so learned and classical a writer. For instance, at page 367, we have this sentence—"The attention of the patient's friends was directed exclusively to the *organ of the lungs.* We ask what is the *organ* of the lungs but the lungs themselves? Should it not have been the "organ of respiration?" Would it not be improper to say, "the organ of the liver," or the "organ of the stomach?"

The 2d species, Icterus Cololithicus, or jaundice from gall-stones, is so closely connected with the genus cololithus itself, as to have rendered it hardly worth Dr. Good's while to separate them. Indeed he only gives the definition of the 2d species, and then refers to the genus gall-stone.

We next come to Icterus Spasmodicus, where, according to our author, "the course of the bile is obstructed by a spasmodic constriction in the course of the bile-ducts." Before making any remark of our own we shall give Dr. Good's description of the symptoms.

"The disease is ushered in by a sense of fulness at the stomach, accompanied with great languor and nausea; a violent pain at the pit of the stomach soon succeeds, with an almost incessant sickness, and an utter inability of retaining either food or medicine of any kind. The pain grows intolerable, and shoots towards the left shoulder, or spreads round the loins, and girds them as with a cord. The epigastric region is greatly distended, and cannot endure the pressure of the hand; while the pulse exhibits little variation.

"The bowels are for the most part costive and moved with difficulty; the urine soon evinces a deep saffron tint, and the sooner in proportion to the violence of the other symptoms, and especially of the retching; and the surface of the body, and especially the fine sclerotic coat of the eye, assumes the same livery. And if the disease become chronic, the yellow dye is not confined to the skin or even to the fluids, but pervades every part of the body, the most compact as well as the most porous; so that the pericardium, the heart, the peritoneum, the meninges, the substance of the brain, the cartilages, and even the bones, are clothed with the common colour. Stoll, Lieutaud, Bartholin, and Morgagni, give various examples of this; though the last observes, that a yellow tinge of the brain is a rare occurrence." 374.

Now Dr. Good acknowledges that these are the symptoms of *cololithus means*, or passing gall-stones; but, says he, "the causes and mode of treatment are different." Yet Dr. Good does not offer us any proof that the causes are different—and we venture to affirm that every practical physician, on coming to a patient with the foregoing symptoms, would pronounce the

jaundice to be from *biliary concretions*, of whatever composition, obstructing the egress of bile from the liver, and thus causing all the phenomena above described. We believe that the ducts of the liver are occasionally constricted from spasm, but that the abovementioned symptoms never occur from such cause alone. If spasm continues long enough to give time for biliary concretions to form, then we have the painful phenomena above described taking place during their expulsion.

Dr. Good believes that jaundiced patients occasionally see objects yellow. This was the case in his own person; but he supposes it only happens when the humours as well as the sclerotic coat of the eye are tinged. The case is certainly a very rare one, notwithstanding the assertion of Lucretius that it is common.

"Multaque sunt oculis in eorum denique mixta,
"Quæ contage suâ palloribus omnia pinxit."*

The following is our author's methodus medendi.

"In the treatment of this species, emetics and cathartics, so highly beneficial in icterus *cholæus*, are of doubtful advantage. Where we have strong reason to suspect that acrimonious materials have formed a lodgement in the ducts or alvine canal, they will prove useful by evacuating them: but in all other cases they must add to the disease by increasing the irritation, and should give way to blood-letting, if the patient be in vigorous health, succeeded by opiates, the warm bath, or warm and anodyne fomentations applied to the epigastrium. The opiate should be given in pills, for the stomach will often for many hours reject liquids of every kind. Two or three grains of the extract of opium may be tried at first, and if this be insufficient, the same or even a larger dose should be repeated half an hour afterwards, and continued till the pain abates. Blistering the seat of pain has been advised by many; and I have often tried it, but without any decided effect. If useful at all, it is rather in preventing a return of the paroxysm than in shortening or mitigating it when present; and will hence be most advantageously resorted to in the interval." 377.

Over biliary concretions we imagine we have very little power. Their expulsion is a work of Nature: but there are many remedies which prevent the formation of these obstructions, by keeping up a due and a healthy secretion of the liver. Of these, the principle are gentle mercurial aperients—the taraxacum—and the nitro-muriatic acid bath—"a remedy which," says Dr. Good, "has, of late years, excited great attention, and is now surmounting an ungenerous prejudice that was at first very extensively raised against it." We have had several proofs in our own practice, of its powers as a preventive of

* De Rerum Natura, IV. 333.

biliary obstructions, and Dr. Good adds his testimony to the same effect. "In two or three instances the advantage has been decisive; and the patients, who had hitherto been seldom two months without a severe return of the complaint, have entirely escaped, and apparently lost, the morbid predisposition." In a few cases it entirely failed in our author's hands.

Of the 4th species of jaundice—that resulting from a scirrhous, tuberculous, or other diseased state of the liver itself, we need say little. The jaundice is a mere symptom or effect of the disease, which too often sets medicine at naught. "Mussabat tacito medicina timore." We shall have another opportunity of alluding to this subject when we come to hepatitis.

The Genus (II.) Melena, is characterized thus:—"The colour of the eyes and skin yellow-green, fuliginous, leaden, or livid; the dejections pale, occasionally dark coloured; anxiety; depression of spirits." This is one of those diseases which have puzzled nosologists as to its place in their systems. Dr. Cullen has omitted it altogether in his Synopsis—briefly noticed it in his First Lines, under hæmatemesis, and, also, diarrhœa—and afterwards introduced it into his "Catalogue of Diseases Omitted, but which ought not to have been omitted." It is divided by Dr. Good into two species—the cholea, and cruenta. The first, which is the black and green jaundice of authors, is thus defined. "Occasional dejections of dark or pitchy bile, intermixed with the fæces; occasional vomitings of yellowish-green and acid colluvies; great languor; often vertigo; hypochondria free from pain, but tender upon pressure." We greatly fear, that Dr. Good has not entirely settled the question of its pathology as follows:

"The liver is here evidently diseased in its structure, and a morbid deep-coloured bile, fulvous, greenish, or fuliginous, is secerned, instead of the natural excretion; from general hebetude and a want of the ordinary propulsive power, it lingers in the biliary passages, if it get into them at all; the finer part of the fluid is first absorbed, and afterwards the grosser; and what remains becomes still more viscid, more stagnant, and of a deeper hue." 389.

The green jaundice occurs more frequently in men than in women—is generally accompanied with disease of the liver, and tenderness on pressure there—pulse natural, or slower than in health—stools irregular, being sometimes pale, sometimes pitchy—urine deeply loaded—appetite irregular. The disease is generally fatal—though its progress is sometimes slow, the patient dragging out a painful existence for three, four or five years. The treatment is undecided. Dr. Baillie, who has written a valuable paper on this subject, thinks he has found neutral salts taken daily as an aperient, of palliative use; but

of a radical cure he seems to despair. Dr. Good has found mercury, combined with antimonials, "highly beneficial," particularly the plummer's pill. To these may be added, alkalies and bitter tonics, and the nitro-muriatic acid, or chlorine bath.

The second species, Melena Cruenta, or "black-vomit," is thus characterized by Dr. Good. "Occasional vomitings and dejections of grumous blood, intermixed with dark-coloured bile; pungent, tensive pain in both hypochondria; compressive pain at the pit of the stomach, and fainting."

"In this species the organs subservient to the formation of bile are in a more debilitated and decayed condition than in the preceding; and it may hence be contemplated as a disease compounded of melæna *cholœa* and hæmatemesis *passiva*, or passive hemorrhage from the vessels of the liver, spleen, or both." 392.

We think it is far from certain what is the source of the melenic discharge. Dr. Good has quoted Drs. Home and Marcard, but he has not noticed a valuable paper on melena by Dr. Cheyne, published in the first volume of the Dublin Hospital Reports, and of which we gave a full account in the first volume of our Quarterly Series, for October 1818, page 200, *et seq.* Dr. Cheyne, in that paper, draws the attention of the profession to the "alternate excess of morbid action in the mucous and serous membranes," by highly illustrative examples of the transference of disease from one to the other of these structures. The disease, whether symptomatic (as in the black vomit of fever) or idiopathic, is extremely dangerous, and no fixed principles of treatment are known.

Genus III. is *Chololithus*, or gall-stone, the symptoms of which are those of icterus spasmodicus. In the disease before us we find certain portions of the bile indurated, and assuming a concrete, often a crystallized form, sometimes of a laminated structure, evincing a tendency towards crystallized rays in the centre, with concentric laminæ towards the surface. Fourcroy supposed these concretions were a resinous matter, combined with a peculiar oil, and a certain quantity of albumen, forming three of the constituent principles of bile. All these principles have been denied by Berzelius, who has discovered that bile becomes resinous only in the process of experiment, by supersaturating it with acids, while the material hitherto regarded as albumen, is nothing more than a small portion of mucous furnished from the gall-bladder! We fear that many supposed constituents of animal and other matters are the "products of experiment," the same as in the above instance. We know, however, that gall-stones are inflammable—that they are found of all sizes in the gall-bladder and ducts—that they are passed with great pain—that they will sometimes lie quiet in the cyst for years, without inconvenience—and that they will occa-

sionally endanger or even destroy life, if lodged long in the ductus communis. Of the treatment we have already spoken.

"Our best and wisest exertions, therefore, must be of a palliative kind, with a view of easing and quickening the passage of the gall-stone. We have no direct means, however, of doing the last: and all we can hope to accomplish is that of rendering a little collateral assistance to the expulsive efforts which are made by nature herself. The duct becomes dilated by the circumambient pressure of the concretion as it gradually passes forward, urged on by the same action that propels the bile in a state of health. Vomiting, therefore, by compressing the whole abdominal viscera, and, particularly, the full and distended gall-bladder and biliary vessels, may afford one mean of pushing forward the concretion; but a gentle force, and consequently gentle vomits, will promise fairer than those which act violently. Dr. Darwin affirms, that in two instances he saw from thirty to fifty gall-stones voided after taking only an oil vomit. If the patient be of tolerable vigour, and inflammation be apprehended, bleeding should precede the exhibition of emetics. Cathartics, by exciting the action of the intestines, and directly stimulating the mouth of the common bile-ducts, contribute, also, to excite action through its entire range, and thus farther favour the expulsion of the concretion. And as we often find its passage evidently opposed by spasmodic constriction, opium, given very freely, and repeated every hour or two, and relaxing the skin by fomentations or the warm bath, will in such cases be of essential service, Horse exercise cannot always be made use of: but where it can be submitted to, it is one of the best auxiliaries we can recommend." 402.

The last genus in the present order is Parabysma, hitherto denominated physconia, but now exchanged for a genuine Greek term, (from παραβυω,) signifying morbid congestion, coacervation, or infarction—"a monstrous race of diseases," as our author expresses it, "which we can rarely hope to conquer, unless we have an opportunity of strangling them in their infancy." This genus comprehends enlargements of the liver, spleen, pancreas, mesentery, intestines and omentum, together with a complication of some or several of these. We shall notice but one or two of them. The parabysma hepaticum arises from various causes, and is attended with a diversity of symptoms, which but seldom enables us to ascertain the exact nature of the swelling ante obitum.

Speaking of the species parabysma hepaticum, helminthicum, or hydaticum, Dr. Good observes—

"As this species of the parabysma depends almost entirely on an atony of the liver, the intumescence increases in many instances in proportion to that atony, and parcularly when debility of the liver is combined with a general debility of the entire system. And hence the liver is frequently known to enlarge in proportion as every other organ becomes torpid and decays. On which account the liver is

often found of an enormous size in dropsical patients. Mr. Gooch gives a case, in which, during dropsy, it acquired the monstrous weight of six hundred and twenty eight pounds.* Baldinger reports another instance in which it reached six hundred and twenty pounds;† and Bonet, a third, in which it weighed only two pounds less."‡ 408.

These amazing enlargements of the liver have escaped our notice, and we have not Gooch's or Baldinger's cases to refer to. On turning to Bonetus, we observe that lib. 1. sect. xviii. is "de oculorum affectibus," and consequently there was no hope of finding enlargements of the liver there. The 16th section of the third book, however, "de tumore hypochondriorum," presents us with many remarkable instances of hepatic intumescence, of which the following, from the language employed, would appear to be the most extraordinary which ever occurred to Bonetus. "Cum itaque opus aggrederer, et circa parenchyma hepatis, tumoris indagandi gratia occuparer, illud cum stupore et admiratione tam vastum ac mole ponderosum videns adstantibus monstro, qui perterriti vix oculis fidem habebant. De pondere autem certior fieri volens, monebam, chirurgum ut extraheret, et totum scirrhosum repertum, pendebat libras xiv. civiles."—*Lib.* III. *Sect.* xvi. *Obs.* 3.

We do not, for a moment, question the accuracy of Dr. Good; but when we consider that a liver, of the size noticed in the extract, must have weighed nearly three stone more than the *whole* of Lambert, "the *greatest* man in England," as the bills facetiously expressed it, we cannot but doubt exaggeration on the part of the original narrators.

We shall not notice the parabysma splenicum, as a professed work on diseases of the spleen is in the hands of one of our coadjutors for early review. And here we must close our first article on the important work before us. The principal defects which we have noticed in this extended publication are the two following:—1st. Dr. Good has not, on several occasions, brought down his pathological information to the most recent periods—a duty incumbent on every one who undertakes a work of this description. 2dly. He occasionally introduces obsolete, irrelevant, or useless materials, when the paramount object ought to be to fill every point of space with the very best, and nothing but the best portions of knowledge. With these trifling defects, which another edition will easily rectify, we have no hesitation in pronouncing the work, beyond all comparison, the best of the kind in the English language. With the naval, the military, the provincial, and the colonial practitioner, the work before us ought, at once, to supersede the unscientific compilation of Dr. Thomas—and it will do so.

* Med and Surg. Obs. † N. Magaz. Band. vii. p. 275.
‡ Sepulcr. Lib. I. Sect. xviii.

Medical and Philosophical Intelligence.

Annals of Name-coining and Medical Style.

We propose to give under this article a list of the new coined terms, and the very learned expressions which we meet with in our reading, and which we think are so injurious to the simplicity of genuine science, that they deserve the severest animadversion. One thing is certain, that they almost uniformly proceed from an inferior source. We can scarcely recollect an instance of any of the great men who have given celebrity and dignity to the profession, having set themselves to the finical task of compounding a multitude of new words from their Greek Lexicons. To avoid the appearance of invidiousness or severity, we shall not give the names of the authors, and we think it would be idle to explain the terms themselves.

Prosophalgia	Phtharmata	Aluropsis
Atrhopuosis	Semeiology	Insufflation
Autalgia	Apocenosis	Synizezis
Hyperœsthesia	Mazology	Retropulsion
Anœsthesia	Embryulcia	Erubescence
Paratrepsis	Putrisolution	Aëroëidic
Pareccrisis	Keratonyxis	Scotomata
Paramorphia	Mercurialize	Necrosomatoscopy
Parrhœmasia	Pupillary aperture	Dacryocystalgia cacochymica
Phymata	Hygienic	

The French instrument for examining the state of the thorax by the sound has been successively called *sonoetre, pectoriloque, thoraciloque,* and *stethoscope!* The Arabians are said to have a hundred names for a horse: we think our scientific language bids fair, at this rate of innovation, to excel them far in the character of useless copiousness.

We quote the following sentence, and a defence of it, and leave our readers to determine its force: "vision may be *hemioptic, ateloptic, myosdesoptic, photoptic, photophobic, oxyopic, nephodic, diplopic, chruptic, strabismal,* or *luscitant.*" "We have not been betrayed," says the writer, "into the use of these, and other appellatives [epithets] by the unworthy influence of affectation or pedantry: our paramount object at all times is, to diffuse the greatest proportion of useful knowledge in the fewest words and the smallest possible space."

An American Professor, writing on Medical Sympathy, says, "The words of the orator strike the ear, and electrify the heart; but have no affinity for the grossness of the humours." "Nothing passes from the speaker to the hearer, but the contagion of the soul." "It is in the magical web of mental sympathy, that Homer and Dante, and Milton and Byron, entangle so indissolubly the souls of their readers, and drag them in triumph at the chariot-wheels of their genius"!—"Terror convulses and shatters the frame with the force of an earthquake." We add, in his own words—"Granted, if you please, for the sake of argument, but not as an offering at the shrine of truth."—

What injury must such bombastic nonsense do to the pupils of Transylvania University, not only in corrupting their taste, but in obscuring the statement of medical facts, in the "lectures and teachings" of this romancing professor.—*Quarterly Journal.*

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Sur l'Emploi, &c.—*On the Employment of Electricity as a Remedy in Concussions of the Brain.* By M. Gondret.—M. Gondret during a residence in the Ukraine in 1819, met with a fatal case of concussion of the brain, from a fall and contusion on the head, in which he regretted that he had not a voltaic apparatus within his reach, as it appeared to him that the electric fluid might have reanimated the individual, in a case where life was merely suspended by the concussion. He resolved, therefore, at a favourable opportunity, to institute some experiments on animals, with the view of determining how far electricity might be serviceable in similar cases. Accordingly, on his return to Paris, he made some experiments, which are here detailed, with their results:—

In the first experiment, he laid hold of a young female rabbit by the hind legs, and gave it two or three blows on the occiput with the palm of the hand. The animal uttered a cry, made some convulsive motions, fell into a state of coma, and exhibited no signs of life, except a slow and convulsive respiration. In this state, nearly approaching to that of death, he established, by means of a galvanic trough of thirty plates, an electric current between the occiput and the different points of the vertebral column. Sudden contractions were immediately produced, but which ceased when the action of the galvanic apparatus was suspended, the animal relapsing into its former state of apparent death, with its eyes dull and immoveable. After waiting for two hours the effect of this proceeding, without any success, he passed the electric stream between the nose, eyes, and auditory canal on one part, and the whole length of the spine on the other, when a general movement very soon began to take place, and the animal made some attempts to walk; and a continuance of this process for half an hour longer, was sufficient to recal it to life. In three hours more the animal eat heartily, and appeared to be quite well. The right eye, which had been principally engaged in the experiment, remained inflamed for three weeks, but recovered its power.

The second experiment was performed on a much stronger female rabbit, and the blows were inflicted with greater force, and, from the violence of the shocks, and the effects which resulted, the countryman who brought the animal conceived it quite impossible to restore it to life, as he was in the habit of killing rabbits for the market with much less violent treatment. The electric stream was kept up in the above manner during four hours, with the effect of exciting only slight convulsive movements, which gradually became more and more feeble. M. Gondret almost despaired of success, when he considered that the epidermis was capable of presenting an obstacle to the free transmission of the electric fluid through the organs; he therefore produced a vesication in the space of a few minutes along the course of the vertebral column, by means of ammoniacal paste. The animal exhibited no signs of sensibility during the action of this topic, but as soon

as the electric stream was re-established between the eye, nose, auditory canal, and the surface which had been ebraded, rapid and general motions of progression and elevation took place. The animal soon raised itself on its fore feet, the posterior extremities not having yet recovered the power of motion. At the expiration of an hour the ears were erected, and the respiration became more frequent; and in this condition the animal was left to itself, the experiment being discontinued, and in the evening it took some food. The posterior parts of the body remained in a paralysed state for three or four days, probably in consequence of the lumbar region having suffered a degree of compression during the first part of the experiment. The right eye, which was at first inflamed, remained in a state of atrophy, and deprived of vision, and the animal did not seem quite well till eight days afterwards.

Both the subjects of the above experiments have since brought forth one or two litters.

The author concludes by suggesting that, provided these experiments are repeated with the same success, it might be advantageous to employ the electric fluid in recent cases of cerebral concussions, and in asphyxia, in combination with other therapeutic agents. The epidermis which covers the face in man, will, on account of its extreme fineness, readily transmit the electric influence, and, consequently, there will be no occasion to engage the eyes, as was done in the above experiments, and it will be no difficult matter to produce a speedy vesication on the trunk or extremities, to allow the electric fluid to pass through these parts.

Magendie adds, that he performed several experiments in conjunction with M.M. Pouillet and Roulin, which correspond exactly with those related by M. Gondret. They found that the voltaic influence was capable of reanimating an animal, which had been immersed under water for more than a quarter of an hour, and which had exhibited no signs of life. But a considerable perseverance was required, for the first return of respiration did not in some instances manifest itself till after half an hour or three quarters of an hour's action of the pile; and from the whole results, he thinks it would be of importance that a powerful galvanic trough should be introduced amongst the means usually supplied by government for the recovery of drowned or asphyxiated persons.—*Medical Intelligencer.*

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Effects of the Section of the Pneumo-gastric Nerve.—M. Dupuy read a series of experiments relating to this subject, which he had made on the horse and sheep. These experiments were repeated by M. Béclard on the dog; and the following are his deductions:—

1st. If we cut the pneumo-gastric nerves of both sides at the same time, the animal dies soon afterwards. 2dly. If we cut them only on one side, life is not affected, and the animal is but little incommoded by the section. 3dly. If, after having divided them on one side, we wait some days and then divide the other, the animal lives a longer or shorter period, in proportion to the greater or less distance of time which is allowed to elapse prior to the second section; and, at the end of a certain time, he lives after the second experiment as if one

section only had been made. 4thly. If, at this last period, he divide both nerves, the animal dies, as in the first experiment.

M. Béclard deduces from these facts, that, at the end of a certain period, cicatrization takes place between the two extremities of the nerve, so as to completely restore its functions.—*Académie Royale de Médicine of Paris, July Meeting.*

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Use of large doses of Tartar Emetic in Pneumonia, Hydrocephalus, &c. &c.—M. Leveillé read a report upon a paper by Dr. Arnaud, of Moulins, in which he relates a great number of cases of peripneumonia and pleuritis, which were cured by the tartar emetic, without having recourse to blood-letting. In support of this observation, M. Laennec communicated some cases of hydrocephalus acutus and pulmonary affections, in which he administered twelve grains and more of emetic tartar during the day, with the greatest success. This latter author made several comparative experiments upon an equal number of cases of peripneumonia, of nearly the same intensity; from which he is convinced, that in those treated solely by bleeding, either local or general, the pulmonary engorgement, discoverable by the *stethoscope*, continued for a very long time; whilst, under the administration of the tartar emetic at a high dose, the engorgement disappeared at the end of a few days. M. Laennec considers that the stibiated tartar, given in this manner, acts specifically, and powerfully promotes absorption.—*Ibid.*

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On some Alvine Concretions found in the Colon of a young Man in Lancashire after Death.—Mr. Children has detailed the case of a young man, who, during the hot weather of July, 1814, was in the habit of eating large quantities of unripe plums, and swallowing the stones, under the notion that they would assist digestion. His health became seriously disordered in February, 1815, and he lingered in great suffering till the 6th of May following, on which day he expired. His symptoms were diarrhœa, pain and tension of the abdomen, emaciation, and a hard circumscribed tumour in the region of the colon. On opening the body, four concretions were found in the arch of the colon, three closely compacted together, the fourth lower, and near the termination of that intestine. Their total weight was nearly five ounces: plum-stones were found in their centres, while the bulk of the concretions consisted of phosphate of lime and ammoniaco-magnesian phosphate, a large portion of animal matter, and a fine fibrous substance derived from the oat, oatmeal having formed a large part of his food during his illness. Mr. Children concludes this paper with detailing the method of analysis which he pursued, and with a reference to some similar cases.—*Philosophical Transactions of the Royal Society of London, for the year* 1822, *Part I.*

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Case in which Three Snails were discharged from the Mouth and Anus; by Dr. Eyting.—Dr. Eyting, of Embden, has published the particulars of this case in the Number of Hufeland's Journal for

April last. The patient, thirteen years of age, had been considerably indisposed; and, from an idea that his indisposition arose from worms, different vermifuges were administered. On paying his visit one day, Dr. Eyting was astonished to find that two snails had been voided, one of which was still living; each of them was an inch long, and three lines thick. Their dorsal surface was black, and the abdominal region white (*Limax atra*). Fearing that others might be retained, he was ordered an emetic, and afterwards a cathartic which last brought away a third, two inches and a half in length, and half an inch in thickness, quite dead. From this time the patient speedily recovered. It was believed that these snails had entered his body whilst he was bathing in some stagnant water, and that their entrance had been facilitated by a prolapsus ani, to which he was very subject.

Were it not that Dr. Eyting has pledged his credit to the truth of this case, and that Hufeland has admitted it into his excellent Journal, we should have been disposed to class this with the well-known case of the *"three black crows!!"*—*Lond. Med. Repository.*

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The Flowers of Colchicum recommended.—From experiments which we are now making with colchicum flowers, we are inclined to conclude, that they furnish the most efficacious and the mildest preparations of this plant. Mr. Frost, who had the goodness to draw our attention to the subject, kindly furnished us with the recent and dried flowers, and with a tincture, a wine, and a vinegar, prepared from them. From the experiments which have been already made, we have concluded, that the "Eau Médicinale" is nothing else than a tincture prepared from the recent flowers. We hope soon to place some interesting matter before our readers on the subject.—*Ibid.*

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On a new mode for the Cure of Prolapsus Ani. By M. Dupuytren.—M. Dupuytren having frequently had occasion to remark the inefficacy of most of the plans adopted for the cure of persons labouring under prolapsus ani, hit upon a mode of cure which he considers as one of the most important inventions in surgery of which he has been the author. This simple operation consists merely "in cutting off a greater or less number of the cutaneous and projecting folds of the verge of the anus: the operation contracts the opening, by drawing it together almost in the same manner as occurs in a purse when the strings are drawn tight." The number of folds which M. Dupuytren removes is proportioned to the size of the protruded intestine, and the dilatation of the anus. Ten or twelve patients have been treated by this method, and all have been cured without any unpleasant symptom or relapse.

Should an artery be opened during the operation, M. Dupuytren immediately cauterizes it. No dressing is required, attention to cleanliness being commonly sufficient to produce the cicatrization of the wounds, and the complete cure of the patients, in less than twelve or fifteen days.—*Journal Universel des Sciences Médicales, Octobre,* 1822.

On the Use of the Preparations of Gold. By Professor LALLEMAND, of Montpellier.—This physician has recently published an essay on the employment of preparations of gold in medicine. He has obtained very speedy and permanent success from the muriate of gold and soda in several individuals affected with inveterate syphilitic complaints, where mercurials had failed. M. L. prefers the salt of gold to mercury, in all those cases where a first mercurial course has been unsuccessful, and *à fortiori*, after a second and third: he has likewise employed it successfully in recent affections. M. L. recommends it to be rubbed upon the gums, tongue, or inside the cheeks. The dose is, at first, a fifteenth or sixteenth of a grain, which may be gradually increased to a fourteenth, twelfth, &c. to a sixth of a grain. Seven or eight grains are commonly sufficient. During the use of the remedy, no remarkable morbid change occurs in the state of health: the gums are not affected by it, and the external characters of the disease quickly disappear.—*Nouveau Journal de Médecine, Octobre*, 1822; *and Journal Universel, Août*, 1822.

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On the Use of Belladonna in Diseases of the Eyes. By NINIAN HILL, M.D., Extraordinary Member Royal Medical Society of Edinburgh.—I beg leave to enclose the case of a young lady, showing the happy effects obtained by the application of belladonna in some diseases of the eye,—a fact not generally known to the public, but of which charlatans successfully avail themselves. From what I have seen and can learn, the itinerant oculist, Mr. Williams, uses the belladonna indiscriminately. The particular effect to which I allude, is very striking in the case of a young lady, with capsulo-lenticular cataract in both eyes, the consequence of an attack of purulent ophthalmia in infancy. The margin of both lenses is nearly transparent, that of the left eye more so than that of the right, for it is by the left only the form of objects is ascertained, while the right is merely sensible of light. The eyeballs have the quick tremulous motion peculiar to congenital cases of cataract. By the aid of very convex glasses, and a shaded light, she with great difficulty was taught to read. The books and other objects were carried close to the eyes, as in myoptic patients. When a distant object was to be looked at, she was enabled to have some idea of its form by arching her hands over the eyebrows. She has now used the belladonna for eight years, and is disposed to think that it strengthens her sight, and that its powers have rather increased. But this I attribute to her having acquired greater dexterity in the application. She applies the solution of the extract four times a day, at each time dropping two drops from a quill upon the ball of the eye; and its effect commences in half an hour from its application. The tremulous motion, I observe, returns, as soon as the influence of the narcotic subsides; and she now depends so much upon it, that she never leaves her bedroom till the pupil is dilated. When I first saw this lady, I applied the belladonna with the view of examining the state of the lens; the effect of which was so powerful, that she was perfectly satisfied,

and declined an operation, to which I cheerfully acquiesced. In most instances, narcotics lose their power; nor is the system so susceptible of their effects after repeated use. But, in reference to belladonna, it would seem fortunately to be an exception; but of which, I must confess, I was not formerly aware.—*Edinburgh Medical and Surgical Journal.*

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Observations upon Excessive Dilatation of the Stomach, without any Obstruction at the Pylorus. Collected at the Hôspital de la Charité, by M. Andral, Jun. M.D.—M. Andral has related two cases of this affection, which are possessed of considerable interest in a pathological point of view; the first occurred in the person of a female, twenty-three years of age, a governess, who had always enjoyed good health. In consequence of some reverse of fortune, she left the country where she had resided, and came to Paris; her health soon became impaired; her appetite diminished; digestion was much deranged; and, towards the month of February, 1820, in consequence of a fright, she was affected with syncope, of considerable duration, after which she repeatedly vomited both her food and drink, some hours after their ingestion: she became gradually emaciated. Leeches were applied to the epigastrium, and magnesia and various antispasmodics given in turn. From the month of December, 1821, the vomiting became more frequent, the strength diminished rapidly, and the catamenia were suppressed. On her entrance into *La Charité*, on the 14th of February, 1822, she was in the last stage of marasmus; the countenance pale, vomiting of solid and liquid aliments more or less speedily after their introduction into the stomach; slight pain at the epigastrium, which was soft, and neither there, nor in any other part of the abdomen, was there any tumour; the appearance of the tongue was natural; constant constipation; pulse very weak, and somewhat frequent; skin dry, and without heat; respiration free. No new symptom appeared until the commencement of March. The emollient drinks which were given were all rejected, as well as the light food which was occasionally allowed her. On the 12th of March, the pulse became accelerated, the tongue dry, and, on the 14th of March, she died.

On dissection, a little fluid was found in each lateral ventricle. Some long and dense cellular bands united the pleura costalis with the pleura pulmonalis. The heart presented nothing remarkable. On opening the abdomen, the stomach was found covering the greatest part of the abdominal viscera, so that some convolutions of the small intestines on each side were alone perceptible; its *colic* edge touched the pubes; the greatest dilatation had occurred at the great *cul-de-sac*. The cavity of the stomach was filled with a greenish yellow liquid; its internal surface was of a rosy tint, and marbled in some parts; on the splenic side it was whiter at this part; to the extent of the palm of the hand the mucous coat was so much softened, as to form a sort of *bouillie* on the least friction; in every other part its consistence was natural. The

parietes of the stomach were generally thin, and easily torn; the muscular coat was remarkable for its great tenuity. The small intestines were very contracted, like those of a dog, and almost entirely contained in the pelvis; their internal surface was pale; the cœcum presented nothing particular. The ascending colon occupied its ordinary place; the transverse followed the great curvature of the stomach, and was found behind it: its internal surface, to the extent of four fingers' breadth, was of a purplish red, seated in the mucous membrane, which was a little thickened. Some red spots, of the same nature, were observed in the descending colon; this intestine was every where much contracted. The sigmoid flexure of the colon and the rectum, which were filled with hard fæces, presented no alteration. The liver extended to the left side, over the spleen; it pressed up the diaphragm to the fifth rib; its inferior part passed a little below the false ribs; its tissue was sound.

The other case, related by M. Andral, is nearly similar. Both of them he ascribes to paralysis of the muscular coat of the stomach. "If we reason from analogy," says he, "we are naturally led to admit its existence: that paralysis of the muscular fibres occurs in several parts, cannot admit of a doubt. Thus the œsophagus becomes sometimes an inert canal, through which the food is only transmitted by its own weight. So the bladder frequently loses its power of contracting upon the fluid which it encloses, and, consequently, is sometimes distended to an enormous extent. In several cerebral affections, the most powerful drastics do not produce any alvine evacuation; it is not that the mucous membrane is insensible at that time to the action of stimulants; for the redness which it presents after death attests that it has experienced the action of the irritants which have been placed in contact with it. Every thing induces me to think, that in these cases the alvine evacuations do not take place, in consequence of the muscular coat having lost its power of contracting. Again, we sometimes find in individuals, who have died of chronic distemper, the small intestines distended by a great quantity of liquid, and their parietes pale, and singularly extenuated. Is there not here, also, a paralysis of the muscular coat.

"It ought to be remarked, that the patient had frequent vomiting; it may be easily conceived that the abdominal muscles had merely to exert a gentle pressure upon the parietes of a stomach, so considerably distended, to provoke the expulsion of part of its contents. It is thus that animals vomit with the greater facility when they have swallowed a great quantity of air.

"What was the cause of the slow consumption under which the patient succumbed? Had the more or less complete deficiency of nervous influence rendered the gastric digestion null or imperfect? for I do not take into account the partial phlegmasia of which the mucous membrane is the seat: in how many individuals do we not find a similar appearance, with scarcely any gastric symptom? Should we not rather admit that chymification was pro-

perly performed, but that, in consequence of the very small quantity of chymified matter which passed into the small intestines, there was no longer a sufficient assimilation?"—*Journal de Physiologie, &c. par* F. MAGENDIE, *Août*, 1822.

Report of a Memoir by M. Flourens, entitled "Determination of the Properties of the Nervous System; or Physical Researches on Irritability and Sensibility;" drawn up by M. Cuvier, the Academy of Sciences having directed M.M. Portal, Berthollet, Pinel, Dumeril, and him, to examine the Work.—They consider it in three points of view; as respecting the author's experiments, his conclusions, and his language. Praise is given with respect to the two first heads; but the author's language is somewhat censured. The irritability of Haller is first described; but it is denied, that Haller positively asserted, that irritability resides entirely in the muscles: for many passages in his works show, that he was not ignorant of the co-operation of nerves in the phenomena of irritability, and Cuvier goes on to say that,

"Now, when it is known, that nerves belong to every muscular part, that a muscular fibre cannot be conceived which is not connected with a nervous fibre, no one will dare to affirm, that the nervous fibre is passive during contraction: all that has been proved is, that contraction can take place independently of all sensation, and of all will, though this sensation may be produced."

Haller's views overthrew the doctrine of Stahl; but before its fall, its defenders changed their views of it, so that they introduced into the language, an innovation which has seemed to make physiology not only the most difficult, but the most mysterious and contradictory of all the sciences. This innovation consists in generalizing the ideas of sensibility, so as even to give that name to every nervous co-operation accompanied by motion, although the animal had no perception of it. Thus were established organic sensibilities and local sensibilities, which were reasoned on, as if they had been the ordinary and general sensibility. The stomach, the heart, the uterus, according to these physiologists, perceive and will; and each organ becomes to itself a sort of little animal, which is endowed with the faculties of a great one. This introversion in the use of terms was singularly favoured, and even augmented, by the double sense which the greater part of the terms had in the French language: in fact, *sensible* signifies at once, what can perceive sensations, what can give them, and what can conduct them. We say in the first sense, that an animal is a sensible being. The second sense is given, when we speak of a sensible noise or a sensible light. The third sense, when physiologists say that the nerves are sensible. It is shown what a deleterious influence this language has had on the science of physiology. The author is then blamed, for using the word irritability, to express the power of a nerve to produce sensation and motion, without perceiving them itself. The nerve is only a conductor of sensation, and a conductor of irritation. M. Cuvier then states the

following questions, which are discussed by the author; first, from what points of the nervous system can artificial irritation be sent to the muscles. Secondly, to what points of the system can the impression be propagated, so as to produce sensation. Thirdly, from what points do voluntary irritation arise, and what parts of the system ought to remain untouched, to produce it regularly. It seems that these questions are only considered as far as relates to the vertebral animals, and to their nervous system of the animal life; that is to say, to the brain, to the spinal marrow, and to the nerves which go from it.

The author first shows, that contractions cannot take place without a free and continued communication between the nerve and the muscle, nor sensation without a like communication between the nerve and the brain. He concludes from this, that neither contraction nor sensation appertain to the nerve—that the two effects are distinct—that they can be produced independently of each other—and these propositions are true, whatever their situation may be, or whatever nervous branch it may be, the communication of which is interrupted. Similar conclusions are come to with respect to the spinal marrow. If it be irritated at any point, contraction is produced in all the muscles, the nerves of which arise below the part, if the communication be free; but not, if it be interrupted. The sensations show exactly the inverse of this; and, as in the nerves, the will requires the same free communication as sensation does, the muscles below the intercepted part no longer obey the animal, nor have they sensation. Again, if the spinal marrow be intercepted in two different parts, if the intervening part be irritated, the muscles, which are supplied with nerves from that part alone, contract; but the animal has no longer any command over them, nor have they sensation. The author concludes from this, that sensation and contraction appertain more to the spinal marrow than to the nerves; and the conclusion is just, as far as entire animals are concerned. Whether it is true of decapitated animals, some of which appear not to lose immediately all their animal functions, will be discussed below. The author concludes also, from a part of his experiments, that it is by the communication which the spinal marrow forms between all the nerves, that the generalization of irritation, or in other words, general sympathies, are formed; but the proposition is not sufficiently made out.

In discussing the properties of the encephalon, the experiments of former authors are censured, as being too vague, because, when pressure was used, the experimenter did not say on what part the pressure was made particularly to bear, nor when the brain was pressed by means of an instrument, through what parts the instrument passed; M. F. therefore, in his experiments, as far as possible, acts, by taking away parts of the brain. We shall translate here M. Cuvier's account of the present state of the physiology of the brain.

"It is now known, and especially by the last researches of M.M. Gall and Spurzheim, that the spinal marrow is a mass of medullary matter, which is white on the outside, and grey within, also divided longitudinally upwards and downwards by furrows; the two bundles of which communicate together, by means of transverse medullary fibres—that it is swelled at intervals; each swelling giving off a pair of nerves—that the medulla oblongata is the upper part of the spinal marrow, contained in the brain; it also giving off several pairs of nerves—that the fibres of communication of its two bundles interlace, so that those of the right penetrate into the left, and vice versa—that the bundles, after having formed one swelling in the mammalia by a mixture of grey matter, and produced the prominence called the pons varolii, separate, and take the name of crura cerebri; still continuing to give off nerves—that they swell a second time, by means of a mixture of grey matter, forming the bodies vulgarly called optic thalami, and a third time to form the corpora striata—that all the external edge of these last swellings gives birth to a layer which is more or less thick, more or less plaited externally, according to the species; entirely covered with grey matter, which passes upwards for that purpose, and forming what are called the hemispheres, both of which are turned in at the middle, and are united to each other by one or more commissures or bundles of transverse fibres, the most considerable of which only exists in the mammalia, and is called corpus callosum. It is known too, that in the crura cerebri, behind the optic nerves, there are one or two pairs of very small swellings, known, when there are two pairs, as in the mammalia, by the name of tubercula quadrigemina, from the first of which the optic nerves appear to rise—that the olfactory nerve is the only one that does not sensibly arise from the medulla, or its pillars—and lastly, that the cerebellum, a mass without uniformity, white within and cineritious without like the hemispheres, but often much more divided by external folds, is placed across, behind the corpora quadrigemina, and on the medulla oblongata; to which it is united by the transverse bundles, which are called crura cerebelli, and which are inserted into the sides of the pons varolii."

It is in these different and complicated masses, that the author has endeavoured to find the part from which irritation proceeds, and at which sensation arrives; and also the respective co-operation of the parts in the acts of the will. He pierced the hemispheres without producing either contraction of the muscles, or appearance of pain: he also cut them away in slices. The cerebellum was treated in a similar manner; and it seems, that both the hemispheres, and the cerebellum, together with the corpora striata, and the optic thalami, were successively removed, but all without effect; it did not even produce either contractions or paralysis of the iris. But when the corpora quadrigemina were pierced, the commencement of trembling and convulsion was perceived; and these effects increased the farther the instrument passed into the medulla oblongata. Strong contractions of the iris were produced, as they also were when the optic nerve was pricked. Lorrey, long ago, came to the same conclusion. Hence, the medulla oblongata and these tubercles, are what M. Flourens calls irritable, but what M. Cuvier calls conductors of irritation, like the spinal marrow and the nerves; but neither the cerebrum nor the cerebellum have this property. The author concludes also, that these tubercles form the continuation and upper termination of the spinal marrow and medulla oblongata: and this is conformable to their connexions and anatomical relations. Wounds

of the cerebrum and cerebellum do not produce pain more than convulsions; and hence these parts are generally considered to be insensible; but the author says, that they are the sensible parts of the nervous system. But this means, simply, that impressions received by the sensible organs cannot produce sensation until they arrive at these parts. The author has proved this position with respect to the senses of sight and of hearing. When the cerebral lobe is taken away on one side, the animal no longer sees on the opposite side, although the iris preserves its mobility. When both lobes are taken away, it loses both sight and hearing; but it is not proved with regard to the other senses. Hence, in place of saying, with the author, that the cerebral lobes are the only organs of sensation, we must be restrained by the facts observed, and content ourselves with saying, that the lobes are the only receptacle of the sensations of sight and of hearing, where they may become perceptible to the animal. We may indeed add, that in this part, all the sensations take a distinct form, and leave durable traces and recollections—in a word, that it serves for the seat of the memory; a property which furnishes the animal with the materials of its judgments. The conclusion is confirmed by the constant proportion which comparative anatomy shows the volume of these lobes to bear, with the quantity of intelligence of the animal. If one of the tubercula quadrigemina be extirpated, blindness and an involuntary rotation of the opposite eye are produced; if both, complete blindness, and a more violent rotation: but the animal preserves all its faculties, and the iris continues to be contractile. If the extirpation be continued so as to touch the medulla oblongata, convulsions are produced. Hence, it is concluded, that the tubercle is only like the optic nerve, a conductor, and that the cerebral lobe alone is the term of sensation, and the place where it finishes, by forming perception. The author's experiments on the cerebellum are considered to be the most curious and original part of his work. When it was cut away in slices, the first removals only produced a little weakness, and a want of harmony of motion; the next slices produced a general agitation,—the animal continued to see and to hear, but its motions were inconsistent and hasty. Its faculties of feeling, walking, &c. were thus lost by degrees; so that when the cerebellum was entirely removed, the faculty of exciting regular movements was entirely lost; although seeing and hearing, and the will to act, remained. The animal had only lost the power of contracting the voluntary muscles; thus, if the cerebrum were taken away, he fell into the state of sleep; if the cerebellum, into a state like intoxication.

"It is surprising," says the author, "to see a pigeon, in proportion as it loses its cerebellum, lose gradually, first, the power of feeling, then the power of walking, and lastly, that of standing upright. Even this last power is lost by degrees: the animal, at first, cannot stand steadily on its legs; its feet then will no longer sustain it; at last a fixed position becomes impossible. It makes useless efforts to arrive at a certain position; and yet, when from fatigue it seems desirous of taking some repose, its senses are so acute, that the least motion makes its exertions recommence; but they are without the

smallest mixture of convulsive motion, as long as neither the spinal marrow nor the tubercles are touched."

Probably no physiologist has ever hinted at this singular phenomenon; and if the discovery be confirmed by repeated and satisfactory experiments, it will bring much honour to the young observer whose work is at present under consideration.

The general deductions from this memoir are, that the integrity of the cerebral lobes is necessary for the exercise of vision and of hearing; and when the lobes are cut, the will is no longer manifested by spontaneous actions. Yet, on being immediately excited, the animal executes regular motions of translation, as if he sought instantly to get rid of the pain and inconvenience. But the motions do not conduct him to the point proposed; very probably, because the memory, which has disappeared with the lobes that formed its seat, no longer furnishes either a base or matter for the judgment. These motions have not the usual continuance, for the same reason; because, the impression which causes them, neither induces recollection, nor a durable will. The integrity of the cerebellum is necessary to produce regularity in the motions of translation. If the cerebrum exist, the animal will see and understand, and have an evident and very energetic will; but if the cerebellum be taken away, it is no longer able to maintain the equilibrium necessary to locomotion. It may be said further, that irritability remains for a long time in parts, without either the cerebrum or cerebellum being necessary. Every irritation of a nerve brings it into play in the muscle in which it resides—every irritation of the medulla excites it in the limbs placed below the irritated point. It is only at the top of the medulla oblongata, at the part at which the tubercula quadrigemina adhere to it, that this faculty of receiving and of propagating, on the one part irritation, and on the other pain, ceases. It is at this part at least, that sensation must arrive to be perceived—it is from this part at least, that the orders of the will must proceed. Moreover, a continuous state of the nervous organs going from this place to every part, is necessary to the excitation of spontaneous motion, and of the perception of impressions whether interior or exterior. The report is terminated by much praise of the author's work.—*Medical Intelligencer.*

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*Uterine Hæmorrhage.**—Although it be a general truth, that uterine contraction secures from uterine hæmorrhage—and that the uterus is contracted when it feels small, round, and firm; yet the attentive practitioner is occasionally struck by the disproportion that exists between the want of contraction and the degree of hæmorrhage—a bulky uterus being often free from discharge—and there being, sometimes, a profuse flow when the uterus is small and contracted.

* An Account of some Circumstances in which a Uterine Hæmorrhage may occur sufficient to produce alarming Symptoms, though the Uterus feels contracted in the ordinary Degree. By Robert Gooch, M. D.—*Med. Chir. Transactions, Vol. XII. Part* 1.

"Nay, further, I have witnessed a profuse hæmorrhage though the uterus had contracted in the degree which commonly indicates security; and I have ventured to do what is seldom justifiable, separate the placenta before the uterus had contracted, without more hæmorrhage than after a common labour. What is this circumstance which has so great an influence that its presence can cause a moderately contracted uterus to bleed profusely, and its absence can cause an uncontracted uterus to bleed scarcely at all?"

Experience has taught our author that, there are two circumstances in which a hæmorrhage may prove alarming, though the uterus be contracted.

1st. The effects of uterine hæmorrhage are comparative in different constitutions. If some people, who are prone to syncope, lose rather more blood than usual, it will affect the constitution as much as a profuse hæmorrhage will others—this trifling excess not being indicated by any thing unusual in the size of the uterus.

2dly. After delivery, in general, uterine contraction prevents hæmorrhage, by closing the mouths of the vessels so as to resist the *ordinary* momentum of the circulation. But if this momentum be *extraordinarily* great, it is reasonable to suppose, that it might overcome the common occlusion of the vessels even in a contracted uterus.

The two positions above stated, are elucidated by cases. The following is illustrative of the first position. Dr. G. delivered a lady of her first child, the labour being short and easy. About ten minutes, *post partum*, the uterus felt round, firm, and of the usual size. In this state of things, she suddenly exclaimed that she was going to faint, (being accustomed to it,) and, in a minute afterwards, she did faint. For a long time, she continued alternately reviving and sinking—"a state which the most experienced cannot watch without painful anxiety." Dr. G. does not give the sequel, but we conclude, of course, that recovery took place.

In some constitutions the syncope is the accidental effect of an exhausting labour, and is accompanied by that powerless and agitated state of the vital functions, termed "nervous irritation"—cases in which the best cordial is an opiate. We shall insert the following case in the words of our author.

"I was attending a lady, thirty-six years old, in her first labour. Of her mother and three daughters, all but one, have, in their first labours, been delivered with the forceps. She had a severe and protracted labour, and no longer feeling justified in postponing the delivery, I applied the forceps. The external discharge was not greater than usual; the uterus, after the removal of the placenta, felt rather, but not very, large. She had not been tranquil since the extraction of the child, but about twenty minutes afterwards her appearance alarmed me greatly. Her heart beat with indescribable rapidity, her pulse was countless, she breathed so quick and short that, to use her own expression, if it became quicker and shorter, she should not breathe at all, and she felt as if she was dying.

"I had two duties to perform; one, to take care that the symptoms were not kept up, nor caused by internal hæmorrhage; the other, to administer what I thought most likely to tranquillize these alarming

symptoms. I passed my hand into the uterus; it contained a good deal, I guess a pound and a half, of coagulated blood; I scooped it out with the hollow of my hand. There was no occasion to irritate the uterus to contract; the instant it was empty it shut like a spring, and when I put my hand on the outside I found it not more than half its previous size, but the symptoms continued as alarming as ever. I now gave her a dessert spoonful of Hoffman's æther, and fifty drops of laudanum, and then sent off for the family physician. When he arrived the symptoms had abated so much, as to relieve me from my anxiety; the pulse was slower, the breathing more tranquil, and she felt disposed to sleep. She continued to sleep nearly two hours while we remained in the house, then awoke for a few minutes, slept well through the night, and awoke the next morning without any vestige of her symptoms, sensible to herself; but her pulse continued frequent for many days." 156.

In illustration of the 2d position, (extraordinary momentum,) the following case is related. On the 10th of April, 1816, Dr. Gooch delivered Mrs. S. of her second child. For many hours before the accession of labour, she was flushed, and had a quick pulse. This state was diminished, but not removed, by proper means. It continued for some time *post partum*. After the removal of the placenta, the uterus felt, in the hypogastrium, contracted in the ordinary degree; yet, in twenty minutes afterwards, there came on one of the most frightful hæmorrhages he ever witnessed. By the introduction of the hand, and the application of cold, it was speedily arrested.

More than a year afterwards, the same lady came to town to be confined, and Dr. G. did not see her till she was in labour. On entering the chamber, he was struck with the same state of the circulation that had preceded her former accouchment—"she was sitting in her easy chair, with a red face and a throbbing pulse." In a few minutes the pains increased and it was necessary to put her to bed. The child was soon expelled, but gradually. The uterus contracted in the usual degree, yet, in a few minutes afterwards, the blood burst out with prodigious impetuosity, and gave rise to a fearful scene. The introduction of the hand and the application of cold speedily arrested the hæmorrhage. For many days afterwards, she could not sit up without faintness.

Our author wisely considered, that if she again became pregnant, it would be prudent to take such measures, before her accouchment, as might cause her to fall in labour with a cool skin and a quiet pulse. In twelve months afterwards, she informed our author, that she was five months gone with child. He advised her to avoid fermented liquors—to take meat only thrice a week—a purgative of salts and senna twice a week—a scruple of nitre thrice a day. This plan was to be begun two months before confinement. Our author saw her four days before her accouchment, and was gratified to find her with a cool skin and quiet pulse. Yet the former inflammatory state recurred, the same as before, for forty-eight hours before labour. The child was gradually expelled—the uterus contracted—but the hæmorrhage came on in a few minutes. It was, however, trifling, compared with the former attacks, and was easily suppressed by wetted napkins

to the belly. In process of time, she again became pregnant, and to the precautionary means above described, she was twice moderately blooded, a little before confinement. Her labour, this time, was unaccompanied by flushing or quickened circulation, and no uterine hæmorrhage ensued.

Our experienced and intelligent author has met with other instances, similar in kind, though less striking in detail. He has seen a strong cordial, given unnecessarily towards the conclusion of a labour, excite inordinate action in the heart and arteries, the consequence of which was a flooding after the separation of the placenta, though the uterus was contracted in the ordinary degree. On the other hand, he has ventured to separate the placenta, while the uterus remained largely dilated; but the circulation being languid, no more blood was lost than after an ordinary labour.

"How often a disturbance of circulation plays an important part in uterine hæmorrhage it is difficult for an individual to know; but I suspect sufficiently often to deserve the especial attention of practitioners. I advise them when they meet with patients subject to hæmorrhage after delivery, to notice the state of the circulation before labour, and, if disturbed, to employ means for tranquillizing it before labour comes on. I advise them during labour, to use cordials cautiously, lest the placenta should separate during an excited state of circulation. I advise them after delivery, though the uterus may feel contracted, to be slow to leave their patient, if the circulation be greatly disturbed." 161.

Dr. Gooch alludes to the important subject of a contracted uterus becoming relaxed, and thus giving rise to hæmorrhage. The states of contraction and relaxation may, and do alternate, with corresponding cessations and recurrences of hæmorrhage. Thus, the first time he attended the lady, whose case has just been related, the bleeding returned again and again, although the abdomen had been covered with pounded ice—the uterus, at one time firm and distinct, becoming at another, so soft as no longer to be felt.

"Finding the ice so inefficient I swept it off, and taking an ewer of cold water, I let its contents fall from a height of several feet upon the belly; the effect was instantaneous; the uterus, which the moment before had been so soft and indistinct as not to be felt within the abdomen, became small and hard, the bleeding stopped and the faintness ceased; a striking proof of the important principle, that cold applied with a shock, is a more powerful means of producing contraction of the uterus, than a greater degree of cold without the shock." 163.

After the second labour, and at the beginning of the hæmorrhage, Dr. G. found the placenta separated and lying in the vagina. He removed it—the hæmorrhage abated, but returned in a few minutes. He took several handkerchiefs soaked in vinegar, and passed them, one after the other, into the vagina, so as completely to plug it. This prevented all external hæmorrhage, and some uterine pains came on. But presently the pains ceased, the uterus softened and enlarged, and she turned ghastly pale. It was now evident, that an external, had been merely converted into an internal, hæmorrhage. He withdrew

the handkerchiefs, and applied his hands in the manner described in the following extract, with perfect success.

"My belief now is, that when hæmorrhage occurs after the removal of the placenta, the quickest way to stop it is to introduce the left hand closed within the uterus, apply the right hand open to the outside of the abdomen, and then between the two to compress the part where the placenta was attached, and from which chiefly the blood is flowing. When the hand is introduced merely as a stimulant, there is an interval of time between its arrival within the uterus and the secure contraction of this organ, during which much blood is often lost. By directing the hand to the very vessels from which it issues, and compressing them as I have described, this quantity is saved. If I may judge by my feeling, the blood stops, in a great degree, even before the uterus contracts; the hand acts first as a tourniquet, then as a stimulant. It is true we cannot tell with certainty where the placenta was attached, and consequently where the pressure should be applied; but as it is generally attached to or near the fundus, if the pressure be directed there, it will generally be right. Besides, after the child is born, it is often several minutes before the placenta separates and descends; if, during this interval, we pass up the finger along the chord and observe at its entrance into the uterus, whether it turn towards the front, the back, the right or left side, or straight up to the fundus, we shall form a tolerably exact idea of the spot to which the placenta has been attached in this individual case." 165.

Dr. Gooch does not consider it necessary during syncope, in such cases, to delay introducing the hand until the patient revives. On the contrary, he introduces the hand, and endeavours to stop the blood, and induce contraction, before giving cordials or stimulants internally.

We have now given our readers a full and complete account of this valuable and truly practical paper. It is a great pity that Dr. Gooch does not write *more*—for very few can write *better*.—*Medico-Chirurgical Review for December*, 1822.